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THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

CONDUCTED BY

SIR ROBERT KANE, LL.D. F.R.S. M.R.I.A. F.C.S.

SIR WILLIAM THOMSON, KNT. LL.D. F.R.S. &c.

AND

WILLIAM FRANCIS, PH.D. F.L.S. F.R.A.S. F.C.S.

“Nec araneorum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes.” JUST. LIPS. *Polit. lib. i. cap. 1. Not.*

VOL. XLVIII.—FOURTH SERIES.

JULY—DECEMBER 1874.

LONDON.

TAYLOR AND FRANCIS, RED LION COURT, FLEET STREET,
Printers and Publishers to the University of London;

SOLD BY LONGMANS, GREEN, READER, AND DYER; KENT AND CO.; SIMPKIN, MARSHALL,
AND CO.; AND WHITTAKER AND CO.;—AND BY ADAM AND CHARLES BLACK,
AND THOMAS CLARK, EDINBURGH; SMITH AND SON, GLASGOW:—
HODGES, FOSTER, AND CO, DUBLIN:—PUTNAM, NEW
YORK:—AND ASHER AND CO., BERLIN.

Meditationis est perscrutari occulta; contemplationis est admirari
perspicua Admiratio generat quæstionem, quæstio investigationem,
investigatio inventionem.”—*Hugo de S. Victore.*

—“Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condât,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu.”

J. B. Pinelli ad Mazonium.

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Page 203, note †, line 3, *for* limited *read* closed.

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[FOURTH SERIES.]

JULY 1874.

I. *On different Forms of the Virial.* By R. CLAUSIUS*.

MY theorem of the virial has already given rise to some discussions on the forms which the virial can assume. I myself, in my first memoir relative to it†, indicated that when the movable points partly exert forces upon one another, and partly are acted on by forces from without, the virial can be analyzed into an internal and an external, and gave their forms for certain frequently occurring cases. Yvon Villarceau subsequently (*Comptes Rendus*, vol. lxxv.) effected other transformations of the equation relating to it, especially by resolving the total motion of the system of material points into the motion of the centre of gravity and the relative motions of the individual points about the centre of gravity, and referring the equation to each of these two constituents singly. Prompted by this, in a note published in the same volume of the *Comptes Rendus* I added a series of further transformations. As, however, in that brief note results only, without demonstrations, could be communicated, and those but imperfectly, a more connected treatment of a subject so important in itself will not be void of interest.

1. The simplest form of the equation in question is the following. If m denotes the mass of a material point which is in stationary motion together with other material points, x, y, z its rectangular coordinates at the time t , and X, Y, Z the com-

* Translated from a separate impression, communicated by the Author, from Poggendorff's *Annalen*, Jubelband, p. 411.

† *Berichte der Niederrhein. Gesellsch. für Natur- u. Heilkunde*, June 1870; *Phil. Mag. S. 4.* vol. xl. p. 122; *Pogg. Ann.* vol. cxli. p. 124.

Phil. Mag. S. 4. Vol. 48. No. 315. July 1874. B

ponents of the force acting upon it, then

$$\frac{m}{2} \left(\frac{dx}{dt} \right)^2 = -\frac{1}{2} Xx + \frac{m}{4} \frac{d^2(x^2)}{dt^2}, \quad (1)$$

or, if (as will always be done in the following) the first differential coefficient according to time be indicated by affixing an accent,

$$\frac{m}{2} x'^2 = -\frac{1}{2} Xx + \frac{m}{4} \frac{d^2(x^2)}{dt^2} (1a)$$

From this results, indicating mean values by drawing a horizontal stroke above:—

$$\frac{m}{2} \overline{x'^2} = -\frac{1}{2} \overline{Xx} (2)$$

If we name the quantity $\frac{m}{2} \overline{x'^2}$ the *vis viva* with respect to the *x*-direction, and the quantity $-\frac{1}{2} \overline{Xx}$ the virial relative to the *x*-direction, since the *x*- is any direction we please, the meaning of the equation can be expressed thus:—*For each freely movable point, the mean vis viva relative to any direction is equal the virial relative to the same direction.*

If we form for a point the equations relative to the three directions of its coordinates and add them up, we get (*v* denoting the velocity of the point, and *l* its distance from the origin of the coordinates):—

$$\frac{m}{2} v^2 = -\frac{1}{2} (Xx + Yy + Zz) + \frac{m}{4} \frac{d^2(l^2)}{dt^2} (3)$$

If, further, we denote by *L* the component, in the direction of *l*, of the force acting on the point, and reckon it positive from the origin of the coordinates onward, the equation (as is readily seen) becomes:—

$$\frac{m}{2} v^2 = \frac{1}{2} Ll + \frac{m}{4} \frac{d^2(l^2)}{dt^2} (4)$$

It is obvious that these equations, which are valid for each individual point, can be extended by simple summation to the entire system of points. We thus obtain:—

$$\sum \frac{m}{2} x'^2 = -\frac{1}{2} \sum Xx + \frac{1}{4} \frac{d^2 \sum m x^2}{dt^2}, \quad (5)$$

$$\sum \frac{m}{2} v^2 = -\frac{1}{2} \sum (Xx + Yy + Zz) + \frac{1}{4} \frac{d^2 \sum m l^2}{dt^2}, \quad (6)$$

$$\sum \frac{m}{2} v^2 = \frac{1}{2} \sum Ll + \frac{1}{4} \frac{d^2 \sum m l^2}{dt^2} (7)$$

introduce a movable system having for its origin the centre of gravity of all the material points, and parallel to the fixed system, and if we name the coordinates of the centre of gravity in relation to the fixed system x_c, y_c, z_c , and the coordinates of any one of the material points in relation to the movable system ξ, η, ζ , then is

$$x = x_c + \xi, \quad y = y_c + \eta, \quad z = z_c + \zeta.$$

If we now form the equation

$$\Sigma m x^2 = \Sigma m (x_c + \xi)^2 = \Sigma m (x_c^2 + 2x_c \xi + \xi^2),$$

and consider that we may put

$$\Sigma m x_c \xi = x_c \Sigma m \xi = 0,$$

we get, if M denotes the total mass of all the material points, consequently the sum Σm , the equation

$$\Sigma m x^2 = M x_c^2 + \Sigma m \xi^2. \quad . \quad . \quad . \quad . \quad (11)$$

In precisely the same manner we obtain

$$\Sigma m y^2 = M y_c^2 + \Sigma m \eta^2. \quad . \quad . \quad . \quad . \quad (12)$$

Finally, the mere substitution in $\Sigma X x$ of $x_c + \xi$ for the coordinate x , X_c denoting the sum ΣX , gives

$$\Sigma X x = X_c x_c + \Sigma X \xi. \quad . \quad . \quad . \quad . \quad (13)$$

If now we form for the centre of gravity the identical equation which for a single material point has served for the derivation of (1), viz.

$$\frac{1}{2} \frac{d^2(x_c^2)}{dt^2} = \left(\frac{dx_c}{dt} \right)^2 + x_c \frac{d^2 x_c}{dt^2},$$

which, after multiplication by $\frac{M}{2}$, can be written thus,

$$\frac{M}{2} x_c'^2 = - \frac{M}{2} x_c \frac{d^2 x_c}{dt^2} + \frac{M}{4} \frac{d^2(x_c^2)}{dt^2},$$

and suppose herein

$$M \frac{d^2 x_c}{dt^2} = \Sigma m \frac{d^2 x}{dt^2} = \Sigma X = X_c,$$

we then obtain

$$\frac{M}{2} x_c'^2 = - \frac{1}{2} X_c x_c + \frac{M}{4} \frac{d^2(x_c^2)}{dt^2}. \quad . \quad . \quad . \quad . \quad (14)$$

With the aid of this equation in conjunction with (11), (12), and (13), the following equation can be immediately derived from (5):—

$$\Sigma \frac{m}{2} \xi'^2 = -\frac{1}{2} \Sigma X\xi + \frac{1}{4} \frac{d^2 \Sigma m \xi^2}{dt^2}. \quad (15)$$

All the equations above derived for the x -direction, of course hold good in a corresponding manner for the other two directions of coordinates; and when each three equations thereby arising are added together, a new system of equations is obtained. In order to write these conveniently, let us introduce the following symbols. We will name the distance of the centre of gravity from the origin of the fixed coordinates l_c ; and the distance of a mass-point from the centre of gravity, λ . Let the velocity of the centre of gravity be called v_c , and the relative velocity of a mass-point about the centre of gravity, consequently the quantity $\sqrt{\xi'^2 + \eta'^2 + \zeta'^2}$, be called w . Further, of the force whose components in the coordinate-directions are X_c, Y_c, Z_c , the component in the direction of l_c may be denoted by L_c ; and of the force acting on a mass-point, let the component in the direction of λ be denoted by Λ . Then the equations will be written as follows:—

$$\Sigma m l'^2 = M l_c'^2 + \Sigma m \lambda'^2, \quad (16)$$

$$\Sigma m w^2 = M v_c^2 + \Sigma m w^2, \quad (17)$$

$$\Sigma L l = L_c l_c + \Sigma \Lambda \lambda, \quad (18)$$

$$\frac{M}{2} v_c^2 = \frac{1}{2} L_c l_c + \frac{M}{4} \frac{d^2(l_c^2)}{dt^2}, \quad (19)$$

$$\Sigma \frac{m}{2} w^2 = \frac{1}{2} \Sigma \Lambda \lambda + \frac{1}{4} \frac{d^2 \Sigma m \lambda^2}{dt^2}. \quad (20)$$

4. We will now turn to the kind of transformation which I communicated in the *Comptes Rendus*, and which depends on the introduction into the formulæ of the mutual distances and relative velocities of each two material points.

First, if ν and μ represent any two of the indices 1, 2, 3, &c., and accordingly m_ν and m_μ are any two of the given mass-points with the coordinates x_ν, y_ν, z_ν and x_μ, y_μ, z_μ , we can form, corresponding to the above, the following identical equation,

$$\frac{1}{2} \frac{d^2[(x_\nu - x_\mu)^2]}{dt^2} = \left[\frac{d(x_\nu - x_\mu)}{dt^2} \right]^2 + (x_\nu - x_\mu) \frac{d^2(x_\nu - x_\mu)}{dt^2}$$

or, differently arranged and written,

$$(x'_\nu - x'_\mu)^2 = -(x_\nu - x_\mu) \left(\frac{d^2 x_\nu}{dt^2} - \frac{d^2 x_\mu}{dt^2} \right) + \frac{1}{2} \frac{d^2[(x_\nu - x_\mu)^2]}{dt^2},$$

which by introducing the force-components is changed into

$$(x'_\nu - x'_\mu)^2 = -(x_\nu - x_\mu) \left(\frac{X_\nu}{m_\nu} - \frac{X_\mu}{m_\mu} \right) + \frac{1}{2} \frac{d^2[(x_\nu - x_\mu)^2]}{dt^2}. \quad (21)$$

Just such equations hold for the other two coordinate-directions; and we will add up these three equations. Therein the distance between the two points shall be denoted by r ; and their relative velocities, consequently the quantity

$$\sqrt{(x'_\nu - x'_\mu)^2 + (y'_\nu - y'_\mu)^2 + (z'_\nu - z'_\mu)^2},$$

we will call u . Lastly, of the forces acting on the mass-points m_ν and m_μ , let the components which fall in the direction of r be denoted by R_ν and R_μ , and at the same time let the direction of force from each point to the other point be reckoned positive. We can then put:—

$$X_\nu(x_\mu - x_\nu) + Y_\nu(y_\mu - y_\nu) + Z_\nu(z_\mu - z_\nu) = R_\nu r,$$

$$X_\mu(x_\nu - x_\mu) + Y_\mu(y_\nu - y_\mu) + Z_\mu(z_\nu - z_\mu) = R_\mu r.$$

Accordingly the equation resulting from the above-mentioned addition takes the following form:—

$$u^2 = \left(\frac{R_\nu}{m_\nu} + \frac{R_\mu}{m_\mu} \right) r + \frac{1}{2} \frac{d^2(r^2)}{dt^2}. \quad (22)$$

Into this we will introduce another simplifying symbol, putting

$$\frac{R_\nu}{m_\nu} + \frac{R_\mu}{m_\mu} = \mathfrak{R}; \quad (23)$$

the equation will then read:—

$$u^2 = \mathfrak{R}r + \frac{d^2(r^2)}{dt^2}. \quad (24)$$

Multiplying this equation by $\frac{m_\nu m_\mu}{2M}$ and extending it to the entire system of points, we get

$$\frac{1}{2M} \sum m_\nu m_\mu u^2 = \frac{1}{2M} \sum m_\nu m_\mu \mathfrak{R}r + \frac{1}{4M} \frac{d^2 \sum m_\nu m_\mu r^2}{dt^2}, \quad (25)$$

wherein the three sums refer to all the combinations of two each of the given mass-points.

5. Between the sums which occur in this equation and the sums previously considered, there are simple relations, which can be discovered by means of a general formula of transformation. For, besides the masses m_1, m_2, \dots, m_n , given two other groups of quantities belonging to them, which shall provisionally

be denoted by $p_1, p_2, \dots p_n$ and $q_1, q_2, \dots q_n$, then the following identical equation holds:—

$$\Sigma m_\nu m_\mu (p_\nu - p_\mu)(q_\nu - q_\mu) = \Sigma m \Sigma m p q - \Sigma m p \Sigma m q; \quad . \quad . \quad (26)$$

in which the sum on the left-hand side refers to all combinations of two masses each, while the sums on the right-hand simply refer to all the masses. A conviction of the correctness of the equation can be obtained by carrying out the multiplication on the left-hand side, and suitably arranging and collecting the terms then contained in the sum. We will now apply this equation to our case by attributing successively different significations to the quantities p and q .

First let us put $p = q = x$; the result is:—

$$\Sigma m_\nu m_\mu (x_\nu - x_\mu)^2 = \Sigma m \Sigma m x^2 - (\Sigma m x)^2 = M \Sigma m x^2 - M^2 x_c^2.$$

We then put $p = q = x'$, and obtain in a corresponding manner

$$\Sigma m_\nu m_\mu (x'_\nu - x'_\mu)^2 = M \Sigma m x'^2 - M^2 x_c'^2.$$

Lastly, we put $p = \frac{X}{m}$ and $q = x$; then comes

$$\begin{aligned} \Sigma m_\nu m_\mu \left(\frac{X_\nu}{m_\nu} - \frac{X_\mu}{m_\mu} \right) (x_\nu - x_\mu) &= \Sigma m \Sigma X x - \Sigma X \Sigma m x \\ &= M \Sigma X x - M X_c x_c. \end{aligned}$$

Just such equations are valid for the other two directions of coordinates; and if we form the sum of each three belonging to one another and divide it by M , we obtain the equations expressing the relations sought, namely:—

$$\frac{1}{M} \Sigma m_\nu m_\mu r^2 = \Sigma m l^2 - M l_c^2, \quad . \quad . \quad . \quad (27)$$

$$\frac{1}{M} \Sigma m_\nu m_\mu u^2 = \Sigma m v^2 - M v_c^2, \quad . \quad . \quad . \quad (28)$$

$$\frac{1}{M} \Sigma m_\nu m_\mu \Re r = \Sigma L l - L_c l_c. \quad . \quad . \quad . \quad (29)$$

Combining these with equations (16), (17), and (18), we get the following very simple equations:—

$$\frac{1}{M} \Sigma m_\nu m_\mu r^2 = \Sigma m \lambda^2, \quad . \quad . \quad . \quad . \quad (30)$$

$$\frac{1}{M} \Sigma m_\nu m_\mu u^2 = \Sigma m w^2, \quad . \quad . \quad . \quad . \quad (31)$$

$$\frac{1}{M} \Sigma m_\nu m_\mu \Re r = \Sigma \Lambda \lambda. \quad . \quad . \quad . \quad . \quad (32)$$

It scarcely needs to be mentioned that in equations (8), (14), (15), (19), (20), and (25), as well as in the earlier corresponding equations, with the formation of mean values the last term (which is a differential coefficient according to time) drops out, and the terms then remaining on the right-hand side are forms for virials, the special signification of which is readily seen in the individual cases.

6. Having thus far been occupied in introducing special quantities of various kinds for the determination of the virial, we will finally derive some equations which, in relation to the variables to be employed, are perfectly general.

Given any variables serving to determine the positions of the points, and denoted by q_1, q_2, q_3 , &c., then the coordinates of the points, and all the quantities determined by them, are to be regarded as functions of these general variables. The velocities, and the quantities determined by them, can accordingly be represented as functions of these variables and of their coefficients of differentials according to time. Let us now assume that the forces acting in our system have a force-function or ergal U , we can treat this as a function of q_1, q_2, q_3 , &c., and at the same time the *vis viva* T of the system as a function of q_1, q_2, q_3 , &c. and q'_1, q'_2, q'_3 , &c. Between these two functions there subsists, according to Lagrange, the following equation,

$$\delta U = \Sigma \left[\frac{dT}{dq} - \frac{d}{dt} \left(\frac{dT}{dq'} \right) \right] \delta q, \quad . \quad . \quad . \quad (33)$$

in which the sum refers to the variations of all the variables q_1, q_2, q_3 , &c. If, for abbreviation, we introduce the symbols p_1, p_2, p_3 , &c., and put

$$p_\nu = \frac{dT}{dq'_\nu}, \quad . \quad . \quad . \quad . \quad . \quad (34)$$

ν signifying any one of the indices $1, 2, 3$, the preceding equation becomes:—

$$\delta U = \Sigma \left(\frac{dT}{dq} - p' \right) \delta q. \quad . \quad . \quad . \quad (35)$$

Besides, according to Lagrange, the following easily derived equation holds for the *vis viva* T :—

$$T = \frac{1}{2} \Sigma p q'. \quad . \quad . \quad . \quad . \quad (36)$$

If we now differentiate according to time the product p_ν, q_ν , we have

$$\frac{d(p_\nu q_\nu)}{dt} = p_\nu q'_\nu + q_\nu p'_\nu,$$

whence results

$$p_{\nu}q'_{\nu} = -q_{\nu}p'_{\nu} + \frac{d(p_{\nu}q_{\nu})}{dt} \dots \dots \dots (37)$$

Herein, for p'_{ν} , we can put an expression to be obtained from (35). For this purpose we will write (35) in the following form:—

$$\Sigma \frac{dU}{dq} \delta q = \Sigma \left(\frac{dT}{dq} - p' \right) \delta q \dots \dots \dots (38)$$

If now the variables q_1, q_2, q_3 , &c. are each independent of the others, their variations are also independent of each other, and the equation which holds for the sum of all the terms must also hold for each term singly; we consequently obtain

$$\frac{dU}{dq_{\nu}} = \frac{dT}{dq_{\nu}} - p'_{\nu}$$

or

$$p'_{\nu} = \frac{dT}{dq_{\nu}} - \frac{dU}{dq_{\nu}} = \frac{d(T-U)}{dq_{\nu}} \dots \dots \dots (39)$$

If we insert this expression for p'_{ν} in equation (37), after multiplying it by $\frac{1}{2}$, we get

$$\frac{1}{2} p_{\nu}q'_{\nu} = \frac{1}{2} \frac{d(U-T)}{dq_{\nu}} q_{\nu} + \frac{1}{2} \frac{d(p_{\nu}q_{\nu})}{dt}; \dots \dots \dots (40)$$

and when we form the sum of all the equations of this kind, we obtain, in accordance with (36),

$$T = \frac{1}{2} \Sigma \frac{d(U-T)}{dq} q + \frac{1}{2} \frac{d\Sigma pq}{dt} \dots \dots \dots (41)$$

These equations (40) and (41) are two new equations representing generalizations of equations (1) and (6).

By forming the mean values, new forms of virial-expressions can be deduced from them. In the first place, the expression for the total virial resulting from the last equation is:—

$$\frac{1}{2} \Sigma \frac{\overline{d(U-T)}}{dq} q + \frac{1}{2} \frac{d\Sigma \overline{pq}}{dt}.$$

In regard to the last term in this expression a special remark must be made. The variables q_1, q_2, q_3, \dots serve for the determination of the positions of the movable points; and, conversely, the values of the variables can be determined from the positions of the points. This latter determination, however, may take place in two ways. It may *have but one meaning*—which is the case for right-line coordinates, the distances of the movable points from one another or from fixed points or the centre of gravity, and for the trigonometrical functions of the

angles made by such right lines; or it may be *ambiguous*—which is the case with the angles themselves, since to one direction an infinite number of angles belong, which differ from one another by 2π . In the former case Σpq is a quantity the value of which, with a stationary motion, varies only within certain limits, and accordingly the mean value of the differential coefficient, taken according to time, of this quantity may at once be regarded as vanishing and be omitted from the above expression. In the latter case, on the contrary, the mean value of that differential coefficient does not necessarily vanish, and hence it must remain in the expression for further consideration.

Should the variables q_1, q_2, q_3 , &c. not be all independent, but connected with one another by certain condition-equations, then we can, notwithstanding, obtain equations similar in form to (40) by employing Lagrange's indeterminate coefficients. Let, namely,

$$\phi(q_1, q_2, q_3, \dots) = 0,$$

$$\psi(q_1, q_2, q_3, \dots) = 0,$$

&c.

be the given condition-equations, we form instead of (38) the following equation,

$$\Sigma \frac{dU}{dq} \delta q = \Sigma \left(\frac{dT}{dq} - p' \right) \delta q + \rho \Sigma \frac{d\phi}{dq} \delta q + \sigma \Sigma \frac{d\psi}{dq} \delta q + \&c.,$$

where ρ, σ , &c. are indeterminate coefficients; and this equation is to be resolved, in the usual way, into as many partial equations as there are variations. The partial equation corresponding to the variable q_v is then

$$\frac{dU}{dq_v} = \frac{dT}{dq_v} - p'_v + \rho \frac{d\phi}{dq_v} + \sigma \frac{d\psi}{dq_v} + \&c.,$$

whence results

$$p'_v = \frac{d(T-U)}{dq_v} + \rho \frac{d\phi}{dq_v} + \sigma \frac{d\psi}{dq_v} + \&c. \quad . \quad . \quad (42)$$

By the insertion of this value of p'_v , equation (37) is changed into

$$p_v q'_v = \left[\frac{d(U-T)}{dq_v} - \rho \frac{d\phi}{dq_v} - \sigma \frac{d\psi}{dq_v} - \&c. \right] q_v + \frac{d(p_v q_v)}{dt}. \quad (43)$$

As many equations of this form are obtained as the given variables q_1, q_2, q_3 , &c.; and the work can be supplemented by eliminating from them the indeterminate coefficients.

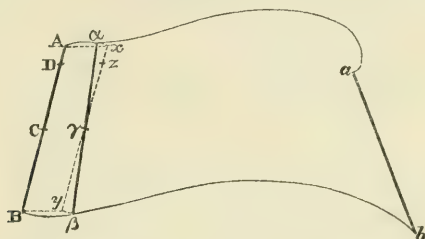
It is thus shown in a general way how the equations which

serve for the determination of the virial can be formed with the employment of any variables; and with this I think I may on the present occasion be satisfied, without entering upon special applications of the equations—which may be of very various kinds, and, hence, would lead to extended discussions.

II. On Amsler's Planimeter. By F. P. PURVIS, Esq.*

THE following is a simple and thoroughly general explanation of the action of this perplexing little instrument.

Suppose, for simplicity and greater generality, that the instrument consisted simply of the straight bar AB , of length l , car-



rying a pencil at each end, A and B ; and suppose any lines Aa , Bb were traced out by these pencils: we will consider how the area $AabB$ may be expressed in terms of l and the motion of some point in the line AB .

Let the motion from AB to $\alpha\beta$ represent an elementary motion of the bar, the centre of it C moving from C to γ , and the bar turning about γ through the angle $d\theta$; let dn = the normal distance from γ to AB : this motion may be considered to take place in two parts:—1st, the motion of AB parallel to itself into the position xy ; 2nd, the motion of AB about its centre into the position $\alpha\beta$; the required area $A\alpha\beta B$ is, in this elementary motion, equal to the area $AxyB$ ($=ldn$), since the area $\gamma\alpha x$ = the area $\gamma\beta y$, and the areas $A\alpha x$ and $B\beta y$ are negligible with respect to ldn , being the product of two infinitesimal quantities, while ldn is the product of one infinitesimal quantity (comparable with each of the two just mentioned) and the finite quantity l .

Integrating for the whole area $AabB$, we see that it is expressed by ln , where n is the travel of the point C normally to the bar AB .

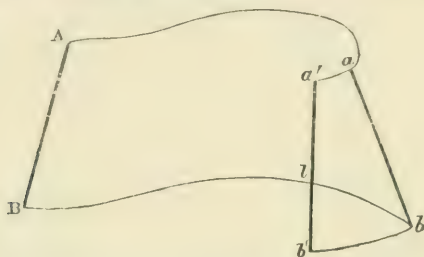
Now we may obtain that normal motion n by centring a wheel on the bar at C , free to revolve in the plane at right angles

* Communicated by the Author.

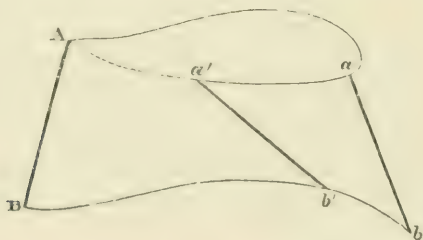
to AB , and resting at its circumference on the paper. That n is given by the circumferential motion of this wheel may be seen by considering again the elementary motion of the bar from AB to $\alpha\beta$: while the bar moves from AB to $\alpha\beta$, the wheel turns through the normal distance from γ to AB ; while the bar turns about the point γ , the wheel remains stationary.

If instead of centring the wheel at C we centre it at any other point D , distant m from C , its circumferential travel for the elementary motion will be the normal from z to $AB (=du) - md\theta$, and for the whole motion from AB to ab will be $n - m\theta$, where $\theta =$ the inclination of ab to AB .

If a retrograde motion be now given to the instrument, bringing it into the position $a'b'$, the product nl will still equal the



area included between $Aa'a'$, $Bb'b'$, and the two straight lines AB and $a'b'$, part of that area being, in the case shown, negative; $n'l = Aa'a'IB - Ibb'b'$. If instead of allowing B to take any path $b'b'$ we constrain it to move only along the line already traced, while A traces out a new line $a'a'$, the negative area will be nil ,



and the product $n'l$ will equal the area $Aa'a'b'B$. If this motion be continued, B being always kept in the path $b'b'B$ until AB occupies its initial position, the product $n'l$ will equal the area $Aa'a'A$, whatever be the nature of the line $Bb'b$. Also for the whole motion $\theta = 0$; so that the circumferential travel of the wheel at $D = n$, entirely independently of the value of m .

Now in Amsler's planimeter the point B is constrained to move in the arc of a circle, while the pencil A is traced round the contour of the required area; this is simply a limitation of the more general case taken above. Also the wheel whose travel is measured is placed away from the centre of the bar C, and indeed on the opposite side of B; but, as we have seen, its position, so long as its centre is on the line AB, is quite immaterial, its motion in the aggregate being the same as if it were placed at C.

In the planimeter the length l is capable of variation; so that, by setting it differently, the same graduation on the wheel will give areas in different units, the unit of area being always $l \times$ the circumferential travel of the wheel required to alter its reading by unity.

III. *On the Polarization of the Zodiacal Light.*

By Professor ARTHUR W. WRIGHT.*

FROM the published accounts of observations upon the zodiacal light, it would seem that few attempts have as yet been made to determine whether or not any portion of the light is polarized, and the results thus far obtained leave the question still undecided. The few notices that can be found in the scientific journals, though uncertain and contradictory, tend to the view that it is either not polarized at all, or that the proportion of polarized light is so small as to render its detection a matter of excessive difficulty. It may be observed that most of the observations giving negative results appear to have been made with Savart's polariscope; but with an instrument which absorbs so large a proportion of the light as a Savart, the amount of polarization necessary to render the bands visible increases very greatly as the light becomes fainter, and especially so as it approaches the limit of visibility. Numerous attempts have been made by the writer to detect traces of polarization with a Savart, but never with the slightest result, excepting that on one especially clear evening, when the zodiacal light was unusually distinct, the bands seemed to be visible by glimpses, on the utmost exertion of visual effort. The observation was so uncertain, however, that it was considered worthless.

Nearly a year ago a series of observations was begun, in the course of which a variety of apparatus were employed, by the use of which it was hoped polarization might be detected, either, as in the Savart, by bands or other variations in the brightness of parts of the field, or as with the double-image prism, the Nicol's

* From Silliman's American Journal, May 1874.

prism, or a bundle of glass plates set at the polarizing angle, by a diminution of the brightness of the object itself. None of them, however, gave results of any value. In resuming the study of the subject some months later, the attempt was made to find a combination which should give a large field of view, and which, while absorbing as little light as possible, should indicate the presence of even small proportions of polarized light, by sufficient variations of intensity to render it available with the faintest visible illumination.

A Savart in which the tourmaline was replaced by a Nicol, though possessing almost perfect transparency, was found to give too small a field of view, and bands too faint to render it of any service. Another instrument was constructed on a plan similar to that adopted by Mr. Huggins in observations upon Encke's comet*, by placing a large double-image prism in the end of a tube 18 inches long, the other end of which had a square aperture a little more than an inch in diameter. The distance was so adjusted that the two images just touched without overlapping. This seemed to promise well; and on using it differences of intensity were perceived which indicated polarization in a plane passing through the sun. Two defects, however, are inherent to this mode of investigation:—one, that if the field is not of uniform brightness throughout, the brighter side of one image may be juxtaposed to the fainter side of the other, thus giving rise to false conclusions; another is the unequal sensibility of different parts of the retina. In consequence of this, the one of the images directly viewed seems always the more obscure, and the true relation of their intensities can only be found by indirect vision, the eye being turned to some point in the median line of the images. Although when used with the observance of the necessary conditions this instrument is capable of giving trustworthy indications, it was soon abandoned for a better.

Among the polariscopic apparatus belonging to the physical cabinet of Yale College, a quartz plate was found, cut perpendicularly to the axis, and exhibiting by polarized light an unusual intensity of colour. It is a macle, the body of the plate consisting of left-handed quartz, through which passes somewhat eccentrically a band of right-handed quartz, 6·5 millimetres in breadth. This band is not bounded by sharp lines of division on the sides, but by intermediate strips (*b, b* in the figures), about 2 millimetres in breadth, which are of different structure, and are apparently formed by the interleaving of the strata of the two portions at their edges. In the polarizing apparatus these strips simply vary from bright to dark, without marked

* Phil. Mag. S. 4. vol. xliii. p. 382.

appearance of colour. Placed between two Nicols, the plate has the appearance represented in the accompanying figures, which are drawn of full size. When the corresponding diagonals of

Fig. 1.

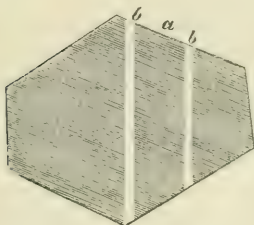
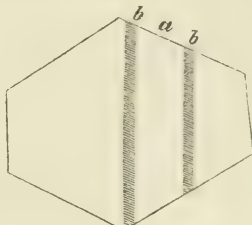


Fig. 2.



the Nicols are parallel, or nearly so, the bands are white upon a deep reddish-purple ground, as shown in fig. 1; with the Nicols crossed, the bands are dark upon a light greenish-yellow background, as represented in fig. 2. Turning one of the Nicols 45° in one direction, the observer sees the central band *a* intensely blue upon a yellow ground; turning in the other direction, a bright yellow upon a dark blue; and intermediate positions give the usual varying tints. Examined with one Nicol and unpolarized light the plate is perfectly colourless, and shows no trace of its heterogeneous structure.

The quartz plate was placed in one end of a tube, large enough to admit its full size very nearly, and 11 inches in length. This was found better than a shorter one, as the bands are most easily seen when not nearer the eye than the limit of distinct vision. In the other end was placed a good-sized Nicol; and the tube was provided with a joint, so that the latter could be easily turned. Thus mounted the plate and Nicol form a polariscope of extraordinary sensibility, with faint light far exceeding the best Savart, and even with strong light somewhat superior to it. The instrument is especially suited for the detection of small degrees of polarization, and the examination of very faint lights. The occurrence of the narrow strips is peculiarly advantageous, as with very feeble illumination they appear bright upon a dark ground, or the reverse, and are thus more easily seen. The efficiency of the instrument is further increased by the comparatively large field of view and the perfect transparency of the whole combination.

As a test of its delicacy may be mentioned that when a glass plate is laid upon the window-sill, and the light of the sky in a clear moonless night, after reflection from it, is viewed through the instrument, both bright and dark bands are easily seen, the former appearing surprisingly luminous in contrast with the

darkened field. The plane of polarization is easily determined with it, since when the bright bands appear, as in fig. 1, the longer diagonal of the Nicol is in that plane; when the bands are dark, the plane of polarization is parallel to the shorter diagonal.

On the completion of the instrument the first favourable opportunity was improved to test its efficiency upon the zodiacal light. It was almost immediately found to indicate the existence of light polarized in a plane passing through the sun. The bands were fainter than had been expected, and at first were overlooked. More careful attention, however, and the observance of suitable precautions established their presence beyond a doubt. The observations were made in a room in the upper floor of one of the college buildings, the windows of which look toward the south-west, and command a clear view nearly to the horizon. The room during the observations received light only from the sky, which sufficed to render objects dimly visible. After being exposed only to this dim light for fifteen or twenty minutes, the eye became sufficiently sensitive for observation. This was a very necessary precaution, as a moment's exposure to a bright light rendered the eye unfit for delicate discrimination of luminous intensities for a long time. The Nicol of the instrument was now turned round and round, so that no previous knowledge of its position relatively to the bands of the quartz plate might influence the judgment as to their character and position. On looking through the tube at the zodiacal light, and turning the whole instrument slowly round, it was possible to find a position where the bands could be seen, and their nature and direction determined. They could rarely be seen steadily by direct vision, and then only for a few moments, as the excitement and fatigue of the eye consequent upon the straining effort of vision soon rendered the field a confused blur. Allowing the eye to rest a few minutes, also on turning it obliquely and rapidly directing it to different parts of the field, and especially by suddenly bringing it to focus upon the quartz plate, the bands could be distinctly seen, and their direction fixed with a good degree of certainty. On the clearest nights the brightest bands (*b, b*, fig. 1) were seen without much difficulty, the broad dark band (*a*), corresponding to an inclination of 45° in the Nicol, less easily, and the dark bands (*b, b*, fig. 2) by glimpses. After determining, by repeated observations, the angle made by each of the bands with some fixed line, as the axis of the zodiacal light, or a line nearly parallel to it drawn between two known stars, the position of the plane of polarization was found, by means of light from a gas flame reflected from a sheet of white paper placed in a suitable position, or by observing the position

of the Nicol. The results of the numerous observations of different evenings were entirely concordant, and showed that the plane of polarization passes through the sun, as nearly as it was possible to fix its direction. In no instance when the sky was clear enough to render the bands visible did their position, as determined by the observations, fail to agree with what would be required by polarization in a plane through the sun. Not the slightest trace of bands was ever seen when the instrument was directed to other portions of the sky.

These observations, for the most part, were made in the ten days preceding new moon in January and February of the present year. During this time there was an unusual number of clear nights, with the atmosphere cold and still. A few good evenings in March and April also were improved in verifying the results previously obtained. The absence of the moon, and the distance of any of the brighter planets and stars from the field of observation, removed all uncertainties from these sources. As the instrument was directed to points from 30 to 40 or even more degrees from the sun, the polarization could not have proceeded from faint vestiges of twilight. That it did not arise by reflection of the zodiacal light itself in the atmosphere, or from atmospheric impurities, is shown both by its amount and the fact that it was always most easily discernible on the clearest nights.

The next step was to determine what percentage of the light is polarized. The failure of the common apparatus to detect it shows that the proportion is not large; but it must be recollected that for a light so very faint much greater differences of intensity are imperceptible than in cases where the luminous intensity is greater. The determinations were made as follows. A bundle of four pieces of excellent plate glass was placed vertically at the centre of the horizontal divided circle of a Deleuil's goniometer, the telescope of which was replaced by the polariscope used in the preceding observations. The latter was so placed that its axis was perpendicular to the surface of the bundle when the index of the goniometer was at zero. With the instrument thus adjusted no bands are seen when unpolarized light is passed through it; but on turning the glass plates bands become visible corresponding to polarization in a vertical plane. The amount of the light polarized by refraction through four glass plates at different incidences has been calculated by Professor W. G. Adams* for intervals of 5° from 10° to 70°, and at 72°. Taking the values given in his Table for crown glass ($\mu = 1.5$), those for intermediate angles are readily determined by interpolation, or graphically. The latter method was employed, a curve being

* Monthly Notices of the Royal Astronomical Society, March 10, 1871, p. 162.

drawn representing all the values in the Table. The results given in the Table correspond very well with those obtained by Professor Pickering*, who verified his values experimentally, and showed that the deviation from theory in the case of four plates only becomes perceptible above 65° . As Professor Pickering used the value $\mu = 1.55$, the numbers in his Table are slightly greater than those used in constructing the curve from Professor Adams's Table.

The determinations were made by observation of the percentage necessary to render the bands visible with the same distinctness as in the zodiacal light. A set of experiments were made with light from the clear sky in a moonless night, the instrument being directed to one of the brightest points of the galaxy, where the light, though less bright than that of the zodiacal light, did not very greatly differ from it in intensity. The glass plates being turned until the bands had the same degree of distinctness as in the previous observations, the mean of several observations gave as the polarizing angle 41° , corresponding to a percentage of 20.5. This value, on account of the inferior brightness of the light compared, is somewhat too large, and may be taken as an upper limit.

To find a lower limit and, at the same time, an approximate value, light reflected from a nearly white wall with a dead surface was employed. The point observed with the instrument was so chosen as to be equally distant from two gas-flames so placed that the planes through them and the axis of the polariscope were at right angles, thus giving light entirely free from polarization. The flames were now turned down equally, so that the field had, as nearly as could be estimated, the same brightness as it had with the zodiacal light. A small scratch upon the quartz plate, which could just be seen by the light of the latter, served as a means of control in adjusting the intensity. The experiments being conducted as before, gave, as the mean of numerous determinations, the angle $36^\circ.6$, corresponding to a proportion of 16 per cent., which is probably not far from the true value of the amount sought. Another, in which the light was made perceptibly brighter than that of the zodiacal tract, gave for the angle $28^\circ.5$, and a percentage of 9.4, which is certainly too small. We may safely take 15 per cent. as near the true value.

The fact of polarization implies that the light is reflected, either wholly or in part, and is thus derived originally from the sun. The latter supposition is fully confirmed by various spectroscopic observations, of M. Liais †, Professor C. Piazz-

* Silliman's American Journal, S. 3. vol. vii. p. 102.

† *Comptes Rendus*, 1872, vol. lxxiv. p. 262.

Smyth*, and others, which show that the spectrum is continuous, and not perceptibly different from that of faint sunlight. The writer has also made numerous observations with a spectroscope specially arranged for faint light, of which an account will be published hereafter, and which lead to the same conclusion. It may be mentioned further that a particular object in these observations was to determine whether any bright lines or bands were present in the spectrum, or whether there is any connexion between the zodiacal light and the polar aurora; and the results give, as an answer to the question, a decided negative. This is important here, as excluding from the possible causes of the light the luminosity of gaseous matter, either spontaneous or due to electrical discharge. The supposition that the light is reflected from masses of gas, or from globules of precipitated vapour, is not to be entertained, since, as Zöllner† has shown, such globules in otherwise empty space must evaporate completely, and a gaseous matter would expand until its density became far too small to exert any visible effect upon the rays of light.

We must conclude, then, that the light is reflected from matter in the solid state—that is, from innumerable small bodies revolving about the sun in orbits, of which more lie in the neighbourhood of the ecliptic than near any other plane passing through the sun. Although such a cause for the zodiacal light has often been assumed as probable, no satisfactory proof of it has hitherto been found; and the establishment of the fact of polarization was necessary to its confirmation, since spectroscopic appearances alone leave it uncertain whether the matter is not self-luminous.

If these meteoroids, as there is no good reason to doubt, are similar in their character to those which have fallen upon the earth, they must be either metallic bodies, chiefly of iron, or stony masses with more or less crystalline structure and irregular surfaces. If we accept Zöllner's conclusion that the gases of the atmosphere must extend throughout the solar system, though in an extremely tenuous condition in space, the oxidation of the metallic meteoroids would be merely a question of time. They would thus become capable of rendering the light reflected from them plane-polarized; and the same effect would in any case be produced by those of the stony character.

In order to ascertain whether the proportion of polarized light actually observed approached in any degree what might be expected from stony or earthy masses of a semicrystalline character with a granular structure and surfaces more or less

* Monthly Notices of the Royal Astronomical Society, June 1872, p. 277.

† *Ueber die Natur der Cometen*, p. 79 et seq.

rough, a large number of substances possessing these characteristics were subjected to examination with a polarimeter. For this purpose the apparatus already described was employed, there being added to it a support for the object, with a horizontal circle for determining the azimuths in placing the object and the light. The substances examined had approximately plane surfaces, which were placed vertically and so that the normal, at the point observed, bisected the angle between the lines from it to the eye and the illuminating flame. The light being thus polarized in a horizontal plane, was depolarized (that is, compensated) by turning the glass plates through the necessary angle, the percentage corresponding to which was immediately found by means of the curve.

If we suppose a line drawn from the place of observation to a point in the zodiacal light, and another drawn from the sun to this at its nearest point, the two lines would meet at right angles; and a surface at the point of intersection must be so placed as to have an incidence of 45° in order to send the reflected light to the eye of the observer. We may in general assume that there would be as many meteoroids on the nearer side of the line from the sun as on the other. Those on the more remote side, while presenting a larger illuminated surface, would reflect the light at a smaller angle, and therefore polarize a smaller amount of it. Those on the earthward side would send less light to the earth, but polarize a larger proportion of it. The differences would so nearly complement one another that we may take their united effect as equivalent to that of a body placed at the point of intersection mentioned above. For this reason the objects tested were so placed that the angles of incidence and reflection were 45° .

Some of the substances, and the percentages obtained, were as follows:—Porphyry, ground smooth but not polished, 35 per cent.; another surface thickly covered with accumulated dust, 15·5; dark blue shale, 25·7; syenite, coarsely crystalline and rough, 16·4; gneiss, rather fine-grained, 8·3; granite, fine-grained, 11·8; red jasper, rough broken surface, 23·5; sandstone 12·1; brick, rough fragment, 8·1; the same, smooth surface, 11·3; red Wedgewood ware, unglazed, 14·2; indurated clay, light brown, 11; mortar, whitewashed surface, 12·1; the same, rough side, 6; white chalk, cut plane, 2. A fragment of the great meteorite of Pultusk, which the writer owes to the kindness of Professor O. C. Marsh, gave from a broken surface 11·7, from the blackened surface, 36 per cent. of polarized light. It is of the stony class, and of a light bluish grey colour.

The results show that from surfaces of this nature the light reflected has in general but a low degree of polarization, not

greatly different, on an average, from that found in the zodiacal light. Although no certain conclusions can be drawn from experiments like these, their results are not inconsistent with the supposition in reference to which they were made, but, so far as they go, tend to confirm it. The results of the investigation may be summarized as follows :—

1. The zodiacal light is polarized in a plane passing through the sun.

2. The amount of polarization is, with a high degree of probability, as much as 15 per cent., but can hardly be as much as 20 per cent.

3. The spectrum of the light is not perceptibly different from that of sunlight, except in intensity.

4. The light is derived from the sun, and is reflected from solid matter.

5. This solid matter consists of small bodies (meteoroids) revolving about the sun in orbits crowded together toward the ecliptic.

Yale College, April 6, 1874.

IV. *The Constant Currents in the Air and in the Sea : an Attempt to refer them to a common Cause.* By BARON N. SCHILLING, Captain in the Imperial Russian Navy*.

INTRODUCTION.

THE currents of the sea and of the atmosphere have been observed from times immemorial ; much has been written on both ; but, unfortunately, science has hitherto made but very unsatisfactory progress in this department. The laws which govern them are still very little understood ; and their originating causes, in particular those of the great ocean-currents and, indeed, of the trade-winds, are as good as totally unexplored, since, on closer examination, every explanation yet given must be regarded as not at all sufficient. A complete knowledge and a comprehensive theory of all currents will long remain impossible, because the currents are subject to the action of very various influences, and these, accompanied by very manifold circumstances, exhibit themselves in such different fashions and are so complicated that it has not hitherto been possible to submit them to exact mathematical analysis. Apart from the theoretical difficulties, practice often opposes insuperable obstacles when we wish to trace a current through the whole extent of its

* Translated from a separate publication communicated by the author, entitled *Die beständigen Strömungen*, &c., Berlin, 1874.

course. The air-currents escape our observations in the upper strata of the atmosphere, and those of the sea in the depths of the ocean. Notwithstanding all the improvements of nautical instruments, we still possess no means of accurately determining the currents at sea. Usually the ship's reckoning (*i. e.* the distance run in a certain direction) is proved from time to time by astronomical determinations of latitude and longitude; and the difference thus brought to light is without hesitation attributed to currents, although it may often result from quite different causes. Although the unsatisfactoriness of this method has long been acknowledged, it is still universally retained for want of a better.

The currents of the sea and those of the atmosphere have hitherto been considered apart—probably because water and air are, in many respects, so very different; but, in spite of this great difference, there can be no doubt that the movements of the sea and of the atmosphere, as fluids, are subject to the same general laws. For, in air as well as in water, gravity is the force which generates currents, because it tends to restore equilibrium wherever it has been disturbed. But equilibrium is disturbed only by the following three principal causes, which, again, are the same for sea and air:—

- A. Alteration of the specific gravity of the water or air;
- B. The rotation of the earth on its axis;
- C. The attraction of the sun and moon.

We see, then, that the currents of the sea and the air depend on the same principal causes, and hence cannot well be separated in the consideration of their theory; only the following circumstances must be kept in view.

1. The air is a highly elastic, readily expanding, gaseous body, while water is almost entirely destitute of elasticity.

2. The atmosphere is heated by the sun principally in the lower strata, causing them to expand, become lighter, and, ascending, communicate their heat to the higher. The sea, on the contrary, is heated by the sun's rays on its surface only to a very slight depth, and, in consequence of evaporation, gives up the greater part of its heat, as latent heat, to the air.

3. In the atmosphere we mostly observe only the currents of the lower strata, and pay little attention to the upper currents, though the latter are often very different, both in direction and velocity, from the lower. In the water, on the other hand, we direct our attention mostly to the upper currents; and only quite recently have temperature-determinations at greater depths begun to throw a scanty light on the deeper currents of the ocean.

4. The seas are bounded by continents, which set impassable

limits to the currents, and thereby exert great influence on their direction, extension, and velocity. It is quite otherwise with the atmosphere, which encompasses the globe, and is undisturbed in its free motion, save, perhaps, in some degree by lofty mountain-ranges. But, on the other hand, currents may arise in the atmosphere through the influence of the interior parts of the continents, to which the sea has nothing to correspond.

Lastly, mention should be made of a certainly only conventional difference between air- and ocean-currents. This is, that the winds are universally named after the point from which they come, while ocean-currents always bear the name of the point toward which they flow. This difference of nomenclature appears at first sight inconvenient; but use has so naturalized these designations in all languages that, as Laughton* quite correctly remarks, any attempt to alter this custom would only give rise to misunderstandings. To impress this upon young sailors, the following phrase is used in the Russian navy:—"The wind blows to the card; currents flow from the card."

The differences just mentioned between the air and water explain why the currents in the atmosphere often appear to us quite different from those of the ocean, since the two classes of currents are exposed to the action of so many and various collateral circumstances that the community of their fundamental laws can almost be no longer discovered. And yet we must be clear about these fundamental laws before we can enter upon the consideration of the collateral actions.

The constancy with which the great ocean-currents and the trade-winds move, and the analogy which prevails between them, justify us in believing that they are less exposed to the operation of secondary causes and, therefore, are especially adapted for the study of the general laws of currents. Hence we will examine singly the above-named three causes, which, to our knowledge, are alone capable of disturbing the equilibrium of the sea and the atmosphere. We will ascertain how far each of them is to be regarded as a generator of the constant sea-currents and the trade-winds, and in what measure it answers to the existing explanations of these currents. The shifting winds and smaller coast-currents we will in general leave unnoticed, because (as already said) our information is still much too limited for us to be able to form even the remotest idea of a theory embracing all currents.

However, before we come to the examination of the forces which call forth the constant currents, we will briefly describe the great oceanic currents and the trade-winds, and indicate how

* *Physical Geography* (London, 1870), p. 176.

the origination of these natural phenomena is at present accounted for.

Constant Currents and Trade-winds. Views hitherto held on their origin.

The analogy which subsists between the constant currents of the different oceans, as well as between the trade-winds and the equatorial currents, is most striking.

Both in the Atlantic and in the Pacific and Indian Oceans there flows from east to west, on each side of the equator, a current extending over about 20 degrees of latitude* and many thousand feet deep. This is named the equatorial current, while for more particular designations the names of the ocean and hemisphere in question are added. Between these two equatorial currents, there is found in all three oceans, nearly on the equator, a relatively narrow zone, in which either no current or one flowing in the opposite direction is observed. The equatorial streams continue their westward course till they encounter coasts, which turn aside their direction, according to the position of the coasts, and give them a more or less meridional direction, until, in both hemispheres, in the vicinity of 40° lat. they turn eastward to intersect the ocean again in this direction. This latter stream, flowing from west to east, pretty well takes in a zone of 10 degrees of latitude, and in all the oceans and both hemispheres is met with between the 40th and 50th parallels. This stream has different denominations in different oceans; but Mührý gives it the general name of the equatorial-compensation stream; for, arrived at the eastern boundary of the ocean in question, the stream turns back again into the equatorial region, to begin afresh its westward course. In this way, in each hemisphere, regular circulations are formed, which are comprehended under the denomination of rotation-currents. In the centre of these circulations, about in the region of the 30th degree of latitude, there is in all the oceans a broad strip in which no current is observed, and which is known by the name of the Sargasso-sea. To these currents parallel to the equator, with their included streamless zones, the trade-winds with their zones of calms exactly correspond. On each side of the equator there is a zone from 15 to 20 degrees broad in which a constant trade-wind blows in the principal direction of east to west. In the vicinity of the polar boundary of this zone the direction of the wind is indeed mostly, in the northern hemisphere, from the north-east—and in the southern, from the south-east. In the middle latitudes, chiefly between the 40th and 50th

* In the Indian Ocean the northern equatorial current, interrupted by the south coast of Asia, has less breadth.

parallels, therefore entirely as with the compensation-currents, constant west winds prevail, under the name of anti-trades. Between these constant air-currents there are, just as with the sea-currents, both in the vicinity of the equator and not far from the 30th degree of latitude in both hemispheres, zones of no wind, which are known by the name of the equatorial and the tropical calms. The trade-winds, with the calm-zones belonging to them, shift a little with the seasons—in the summer of the northern hemisphere moving somewhat northward, and in the winter toward the south. In the currents of the ocean this shifting of the zones is less marked; hence the currents of water and air do not exactly correspond; yet, excepting slight deviations, the trade-winds with their calms present the same picture as the equatorial currents with their streamless zones. In spite, however, of this striking similarity, the trade-winds have till now been ascribed to quite different causes from those of the great rotation-currents of the ocean.

We shall presently return to this subject; we will now only mention further that the rest of the great currents in the different oceans likewise correspond so completely that the existence of very determinate laws must thence be inferred. Thus the Gulf-stream and the Japanese Kurosiwo exhibit precisely the same phenomena: both are currents of warm water, flow in a north-easterly direction, and are separated from the coast to the west of their course by a cold current flowing in the opposite direction. The cold current of Peru corresponds to that of South Guinea, just as does the warm Brazilian current to that of Mozambique. Lastly, a feeble current from south-west to north-east prevails in the entire south polar sea.

The trade-winds have from the 17th century been represented as polar winds which are deflected from the direction of the meridian by the earth's rotation: this theory was, as far as we know, first set up (very imperfectly, it is true) by Varenus, in 1650; it was subsequently improved by Halley in 1686, and Hadley in 1735, and is for the most part named after the latter, as it has since made no advance. That this theory has been so generally accepted is so much the more surprising, as many phenomena of the trade-winds are scarcely in accordance with it.

According to this theory, the masses of air in the equatorial regions, rendered lighter by heating, are continually ascending, through which the cooler and heavier air of higher latitudes is impelled to flow toward the equator. As the velocity of rotation is greater at the equator than at any other latitude, and gradually diminishes to the poles, while the air-particles (by the law of inertia) do not at once take up this greater velocity as soon as they arrive at parallel circles where the motion is more

rapid, the polar wind is turned westward, and expresses itself in the northern hemisphere by a north-east, in the southern by a south-east wind. In the higher strata the ascending air returns to the poles to serve as a compensation for the air which has flowed thence to warmer latitudes. As this upper anti-trade streams polewards, it receives from the rotation of the earth a deflection eastward in both hemispheres. Compressed by cooling and the polar convergence of the meridians, it sinks at about the latitude of 30° to the surface of the earth and so forms the constant west wind of the middle latitudes. The ascent of the air at the equator and its descent in 30° lat. will produce in the first case the equatorial calms, and in the second the calms of the tropical zones.

This is, as briefly as possible, the generally recognized theory of the trade-winds, which, however, is not at all adapted for explaining the perfectly analogous rotation-currents of the ocean.

The equatorial current is still regarded by many as a drift-stream produced by the trade-winds. This already long-persistent opinion received such a confirmation by the authority of Franklin and Rennell, that, notwithstanding its forcible refutation by Maury and Mühry, it is still maintained, although only in England. For instance, Herschel, Carpenter, and Laughton have recently pronounced in favour of this explanation. Far more prevalent, however, is now the view that the cause of the equatorial current is to be sought immediately in the axial rotation of the globe. Columbus, the discoverer of this current (in 1492), accounted for it by the universal motion of the heavens (*con los cielos*) from east to west*. This notion of the "*primum mobile*" was followed by all, till Kepler at the commencement of the 17th century pointed out, and Varenus (1650) proved in detail, that the current was occasioned not by the "*primum mobile*," but by the rotating motion of the earth, the water not being able to keep up with the earth's rapid motion. Mühry, the chief authority on ocean-currents, substantially shares this view, only giving to it a different and not quite intelligible expression. He says†, as Fourier‡ before him, that the staying behind of the water is effected by the centrifugal force of the earth. By this expression we are accustomed to understand the force that throws off from the centre, which always acts in the direction of the radius of each parallel circle; and hence we cannot see how this force could cause the swiftness of rotation of the water to be less than the rotation-velocity of the entire globe.

* Kohl, *Geschichte des Golfstroms*, p. 30.

† *Ueber die Lehre von den Meeres-Strömungen*, p. 5.

‡ *Ann. de Chim. et de Phys.* 1824, p. 140.

On the other hand, Mühry accounts for the compensation-current, flowing in the middle latitudes from west to east, by the aspirating or attracting force of the equatorial current—that is, by the tendency of water to find its level, which impels it to fill up the void caused by the primary current. He therefore makes the aspirating force operate in a vast arc across the entire ocean, and chiefly between the 40th and 50th parallels of latitude—a decided *circulus vitiosus*.

The meridional currents* are mostly accounted for by the constant difference of temperature of the equatorial and polar regions; and Mühry attributes to the cold and heavy polar flow the primary, and to the warmer and lighter compensating antipolar flow the secondary action. At the same time, in consequence of the velocity of the earth's rotation progressively diminishing in the direction of the poles, all the cold or polar streams receive a deflection of their direction to the west, and the warm antipolar currents a deflection eastward. Franklin and Rennell explained also the meridional currents by the action of the trade-winds; for they believed that by the driving force of these winds the waters are accumulated in the Gulf of Mexico and are discharged in the Gulf-stream,—a view that probably now possesses scarcely any adherents.

Having briefly indicated the existing explanations of the origin of the great ocean-currents and the trade-winds, we will now endeavour to ascertain in what manner each of the three forces before mentioned is capable of acting upon the currents, how their influence must express itself, and, finally, how far the explanations hitherto given correspond with the facts.

A. ALTERATION OF THE SPECIFIC GRAVITY OF THE WATER AND AIR.

a. *Difference of Temperature.*

Every material substance possesses the property of occupying a greater space when its temperature is raised, while still retaining the given number of its molecules and its weight. From this it follows that, after a rise of temperature of a body, fewer of its particles can find room in a given space; so that the specific gravity must diminish with rise of temperature. Substances of different kinds differ widely in their degrees of expansion, and hence also in the alteration of their specific gravity. According to determinations by Erman†, sea-water expands 0·00027 of its volume with every degree between 0° and 12° R. On this ground it has been calculated‡ that the entire mass of

* These, which flow in the direction of the meridian, Mühry calls latitudinal.

† Pogg. Ann. vol. xx. p. 114.

‡ Bischof, *Lehrbuch der chem. u. phys. Geologie*, p. 7.

equatorial water would stand 14 feet higher than the water of the polar seas, if it could not flow off. It has been thought that this tendency of the equatorial water-surface to rise would serve to account for the Gulf-stream, which accordingly would flow down hill. But this inconsiderable elevation of the surface of the equatorial sea would not give a fall of even $\frac{1}{4}$ inch in a German mile, which, in relation to the velocity, is much too little. Even the assumed elevation of the surface, however, can never actually be produced; for as soon as any particles of water become a little lighter, they must, in obedience to the law of gravitation, immediately spread uniformly over the entire surface. Thereby is necessarily produced a flow of the warmer and therefore lighter superficial water to the colder regions, and of the heavier cold water at the bottom to the warmer regions. Such an exchange of the waters of the warmer and colder seas exists in reality. A proof of this is furnished by the temperatures of the ocean diminishing with increasing depth—the temperatures of the greater depths being very low, even in the equatorial regions. An exception to this rule is formed by those seas which are divided from the ocean by a ridge over which the water is considerably less deep. In such seas the temperature sinks merely to a depth corresponding to the height of the water above the ridge, and below that remains nearly unaltered, because the colder water, cut off by the ridge, can have no influx. We have an example in the Mediterranean, united with the ocean by the Straits of Gibraltar, the depth of which is only a little over 100 fathoms,—and in the Skagerrack and some Norwegian fjords, for which the bottom of the proportionally much shallower North Sea forms a ridge. It is therefore unquestionable that water flows at the surface of the sea out of warmer into colder, and in its depths out of colder into warmer regions; so that there only remains to get an idea of the velocity of these currents.

Water, as a bad conductor of heat, is warmed only very slowly, and expands just as slowly. Now, as this expansion is moreover very trifling, the streaming produced by difference of temperature must likewise be only an extremely slow, creeping motion.

In order to give an idea of the origination of the meridional currents from difference of temperature, Dr. Carpenter showed, on the 9th January 1871, at the Royal Geographical Society in London, the following experiment. He filled a glass tank, several feet long, with water, which at one end of the tank he cooled with ice, and at the other end, by means of a special arrangement, he heated at the surface. The cooled water was coloured red; the heated, blue. At the close of the lecture,

which may have lasted an hour, the blue water had moved along the surface, and the red along the bottom ; but, notwithstanding the pretty considerable difference of temperature and the length of time, the coloured water particles had travelled only a few feet. This experiment proves, therefore, only what we have already said—that through difference of temperature an exchange of the water particles must take place, but that this exchange proceeds very slowly, even with considerable difference of temperature and with little distance between the differently heated waters.

In nature, however, the difference of temperature of sea-water is proportionally inconsiderable, never amounting in the whole to more than about 30° C., while this difference is distributed over the vast distance of the polar from the equatorial seas. It thus appears, then, impossible that this cause can have power to set in motion such a current as the Gulf-stream. Even the mass of the heated water, which is so readily invoked, cannot here exert any accelerating action, because only an inconsiderable superficial layer is warmed by the sun, and nothing hinders the direct, gradual, and immediately complete interchange of the water particles. Only when large basins of water of different temperatures are united by a channel can the mass of the warmer water play a part, and the difference of temperature generate a considerable current in the channel. Thus, for example, we may regard the northern part of the Atlantic between Norway and Greenland as a broad channel connecting the north-polar basin with the ocean.

The air, however, expands 15 times as much by heating as water, and the influence of temperature-difference on air-currents is undeniable ; yet even here that influence is generally very much overrated. For air, as well as water, is a bad conductor of heat, and therefore only slowly changes its temperature and therewith its specific gravity. Hence we are decidedly of opinion that the expansion of air by heating in the open can at most occasion only a gradual inflow of air, never a sudden gust or a rapid fall of the barometer. The gradually heated air rises only gradually and slowly, and is just as slowly replaced by cooler air. A burning light, or a chimney-fire gives us the best proof of the correctness of this assertion. Although the temperature produced in the fireplace is incomparably higher than ever occurs in nature through the heat of the sun, and the draught is artificially increased by the height and narrowness of the flue, yet this draught is so inconsiderable that it can seldom carry up a piece of paper thrown into the chimney, and even the ashes of burnt paper are hardly lifted. This shows us that even a strong heating occasions only a slow ascent of the

air; so that by it, at all events in ordinary circumstances, a breath of air, but no wind, can be produced. In a forest-conflagration, the enormous heat appears to cause a considerable ascending current of air; but even then the inflow of air thereby effected is perceptible only in the immediate vicinity of the burning.

Hence we believe that wind can mostly arise only from condensation of the aqueous vapours in the atmosphere, which possess the property of very suddenly changing considerably their degree of elasticity and the pressure resulting from it; this must, of course, exert a great influence on all atmospheric phenomena. Certainly the elasticity of the aqueous vapour stands in the closest relation with the temperature, which so far, therefore, operates indirectly in the origination of wind. Whether change of temperature is the only cause of the condensation of vapour, we know not: as, when the aqueous vapours are condensed, electricity is always set free, perhaps conversely the condensation may be caused by electricity.

At any rate it is changes in the tension of the aqueous vapour that produce strong winds; and only in very peculiar cases can the, by itself, slow expansion of the air develop stronger winds. Thus, for instance, when the greater portion of a continent is powerfully heated by the sun, the mass of air rising, though only slowly, from the whole of the vast surface, would require for its replacement (that is, for the restoration of equilibrium) such a mass of air that the inflow must be much accelerated, because it forms a stratum of little height in comparison with the magnitude of the heated surface. An example of winds thus produced is afforded by the monsoons. Over the ocean the atmosphere can never be so much heated as over the land; and, besides, evaporation of the water of the sea and the elasticity of the aqueous vapour are augmented as the temperature of the air rises. If, therefore, the expansion of the air diminishes the atmospheric pressure, on the other hand the augmented elasticity and quantity of aqueous vapour will again increase it; and it is difficult to decide which of these two may exert the greater influence. Admitting that the diminution of the atmospheric pressure by expansion of the air is greater than the rise of the pressure by aqueous vapour, it yet appears to us self-evident that the wind resulting from the heating of a continent must be much stronger than that produced by the heating of the air over the ocean, supposing both to take place over very considerable spaces.

If, then, the recognized theory of the trade-winds were correct and they were produced by the ascent of the heated air, the trade-winds would blow in summer towards the Sahara, since

there the thermometer not seldom shows 50° C. in the shade, while in the equatorial regions of the ocean the temperature of the air never rises above 30° C. The trade-wind, however, at the north-west coast of Africa blows constantly from the desert, carrying the fine sand far out to sea.

Further, in summer the temperature of the 20th and 30th degrees of latitude is not lower, indeed it is higher than that of the equator, and yet the shifting of the trade-zones is inconsiderable, and the wind keeps its usual direction.

Likewise, according to Hadley's theory an ascending or a descending current should prevail in the calm-zones. Now this current must be very considerable, if it brings forth the fresh-blowing trades and anti-trades; and the ascent of the air in the equatorial calms, and its descent in the tropical calms, would make themselves perceptible, even if the motion were very slow. This, however, is not the case: a particle of dust loosened from the sails falls, both in the equatorial and the tropical calm-zone, quietly to the deck, without exhibiting the slightest tendency to be impelled upward or downward. From this we conclude that the upward and downward currents of air in the zones of calms, if they really exist, must be so slight that the generation of the trade-winds and anti-trades cannot possibly be ascribed to them.

Let it further be remembered that in the middle latitudes of the northern hemisphere the anti-trade often blows from the north-west instead of south-west, and in the southern hemisphere from south-west instead of north-west—which could not be, if, as required by Hadley's theory, it formed a current directed toward the poles.

Lastly, in Central Europe these constant west winds blow in summer very moderately, while in winter their force is much increased—which again does not correspond with the theory; for in summer the eastern steppes are strongly heated and should attract the west wind, while the cold which prevails in Eastern Europe in winter would, on the contrary, contribute to the weakening of the west wind.

All this and many other reasons* show that the existing theory of the trade-winds is not sufficient to account for the phenomenon, and that another must be sought.

We do not on this account dispute that heated air must ascend; we only believe that, since the heating and expansion proceed very gradually, the ascent must also be very slow and hence cannot constitute the principal cause of the trade-winds. Their main motive cause appears therefore to lie in other forces, of which we will speak subsequently.

* Laughton, 'Physical Geography,' pp. 120-127.

If, then, difference of temperature can only call forth inconsiderable winds by the expansion of the air, it is clear that in water, so much less expansible, heated to only a proportionally slight depth, no great current can be generated by this cause. If this force were sufficient to occasion a considerable current, it would extend over the entire surface of the ocean, and not merely show itself in a narrow strip at its margin.

We nevertheless allow that the heating of the water may, in certain cases, have an important influence on the maintenance and extension of an already existing current. If, *e. g.*, a current arising from other causes strikes upon a coast, it usually takes the direction of this coast, along which it continues until it gets beyond the sphere of the action to which it owes its development. But if it accumulates at the coast the heated surface-water of the sea, the mass of warmer and lighter water, continually replaced, will perpetually exhibit the endeavour to spread over the heavier water of colder regions. Difference of temperature will therefore in this case have an essential influence on the continuance and the direction of the currents toward higher latitudes, but cannot independently generate the currents. This, then, explains to us also how it is that warm and cold currents are found preeminently at sea-coasts. The first impulse, however, to the flowing which collects the heated water does not arise from difference of temperature, but always from other causes. The ascertaining of these initiating causes is of very great importance for the foundation of a theory; for without accurate knowledge of the fundamental laws, we can get no account of the action of the accessory causes.

Let us now consider the possible influence of evaporation. As already said, the evaporation of water is in close connexion with heat; for with a rise of temperature the capability of the air to take up aqueous vapour is also increased. Hence much more water is evaporated in the equatorial regions than in higher latitudes; and the vapours are driven by air-currents into other, cooler regions, where, on the cooling of the air, they are given back to the sea as an atmospheric precipitate. Evidently from this cause must arise a sea-current toward the equator, though only a very inconsiderable one. Mühry calculates that in the tropics about 15 feet of water evaporate yearly, therefore about half an inch daily. Perhaps half of this evaporation is returned to the tropical seas as rain and river-water, and only the other half ($\frac{1}{4}$ inch daily) returns by sea-currents from higher latitudes. But a current which in the course of 24 hours replaces only a layer of water a quarter of an inch in thickness, must be imperceptible. This slight current flows at the surface, and is directed to the equator; it thus counteracts the current

arising from difference of temperature, which, as above remarked, must flow at the surface from warmer into colder zones.

Evaporation, then, also cannot occasion any perceptible current in the open sea; but in channels connecting an inland sea with the ocean a difference of level between the two seas, arising from greater evaporation of the inland sea, may occasion a strong current. We have instances of this in the Straits of Gibraltar and Babelmandeb.

Let us now turn to the consideration of the influence which heat may have indirectly on the origination of sea-currents when, through its action upon the aqueous vapour in the atmosphere, it generates wind. The action of wind upon the surface of water is familiar not only to the inhabitants of coasts, but to almost every one. Indeed we see in every pond how the water is driven by a strong wind; and if the basin is flat and not very deep, not seldom does the water recede from the windward side, and accumulate on the lee side. Such heapings-up of the water in shallow bays, and at the mouths of great rivers, by strong winds occasion inundations. At a straight coast-line, too, the water may rise considerably by the force of the wind, if the depth increases very gradually and thereby the outflow beneath is checked.

Indeed, on the open sea the wind often drives the water before it, and thereby forms what are called drift or superficial currents; but these, as irregular phenomena, do not here come into consideration; we can only occupy ourselves with those currents which are called forth by constant winds, *i. e.* the trade-winds. Even the constant action of the trade-winds, however, is hardly able to occasion any very deep-going current, as has already been sufficiently shown by Maury and Mühry.

According to FitzRoy's data, the highest waves rise to the height of 60 feet*, measured from the trough to the crest of the wave, therefore 30 feet above the smooth surface of the sea. If we might assume that the entire wave could be driven forward by the wind (which is decidedly assuming too much), thus would be produced a current of 30 feet depth. Through the friction of the water-particles, the effect of the wind upon the current may become sensible somewhat deeper still; but the

* This number appears to us very high; for we have often, in a severe storm, ascending the shrouds, tried to bring our eye into such a position that, at the moment when the ship was exactly in the trough, we could see several wave-crests in a horizontal line. The height of the eye above the ship's water-line then determines the greatest height of the wave. In this way I have only once at Cape Horn (where the waves rise uncommonly high) measured a height of 46 feet, and at the coast of Japan, in a typhoon, one of 38-40 feet; at other times the height was mostly less than this.

flowing must rapidly diminish downward, and soon entirely cease. A. Findlay* thinks that the wind can never call forth a current of greater depth than 5 fathoms. James Croll's remark†, that the duration of the wind, as well as its force, must have great influence on the depth to which it acts, may to a certain extent be quite correct; but, nevertheless, action of the wind upon currents at a depth of thousands of feet (as, for instance, in the equatorial current) is not possible; hence we must see that Franklin and Rennell's view, that the equatorial current results from the action of the trade-winds, cannot be true.

Nature herself gives us decisive proofs against that view. With the shifting of the zone of calms it happens that the equatorial current flows just as well in that zone as in the trade-wind. In the Indian Ocean the change of the monsoons has scarcely any influence on the equatorial stream. The Gulf-stream often flows in opposition to very violent storms, which would be impossible if its motive force lay in the trade-wind.

Even the opinion that the trade-wind raises the level of the Gulf of Mexico, and so produces the Gulf-stream, is untenable. In the first place, it is proved by the levelling of the isthmus of Panama and the peninsula of Florida that this is not the case, as the level of the Mexican Gulf pretty closely accords with both that of the great ocean and that of the Atlantic. Secondly, in the open sea an enduring higher level can never be produced by the action of the wind; for as soon as any particles of water, driven by the wind, change their place, they compel by their pressure just as many others immediately to take the place left free by them. Only where the formation of the coast hinders, or at least impedes, this back-flowing motion, can an alteration of level take place.

Certainly the mechanical pressure of the wind upon the surface of the water can, in the open sea, somewhat alter the level; but as the wind mostly acts horizontally, or at a very acute angle, upon the surface, the mechanical pressure is so little that the oscillations of the sea-level brought about by it must likewise be inconsiderable.

Just so must the variations of the atmospheric pressure exert an influence on the height of the water of the seas, and consequently also on their currents. When, for example, the barometer falls an inch, the surface of the sea at the place must rise 13·6 inches, and *vice versa*, and thus a current be formed from where the pressure is high to the region where lower pres-

* A Dictionary for Navigation of the Pacific Ocean (London, 1851), vol. ii. p. 1222. Also Mühry, *Geograph. Mittheilungen*, 1872, p. 136.

† Phil. Mag. October 1871, p. 268.

sure prevails. It is self-evident that a current thus produced in the ocean can only be very inconsiderable and inconstant, since it changes with every change of the atmospheric pressure. This force, again, can only make itself perceptible in channels.

Suppose that on an inland sea connected with the ocean only by a strait the barometer suddenly fell 1 inch, the level of this sea would stand 13·6 inches lower than equilibrium with the ocean at the moment would require. The mass of water wanting must therefore pass from the ocean through the strait.

We must ascribe it to this circumstance that, in the Sound, a change in the direction of the current mostly occurs 24 hours before a change in the direction of the wind. Just so the water usually rises in the Gulf of Finland before the south-west wind commences; and this rise is noticed also in winter, when the entire surface of the sea is covered with ice, and thereby the direct action of the wind is withdrawn.

b. Saltness of Sea-water.

Alterations in the saltness of seas greatly affect the specific gravity of the water. The observations, however, which have been made in different parts of the world have proved that the difference in saltness of the various oceans is extremely slight*. This compels us to the conclusion that currents immediately tend to equalize the slightest difference in the saltness of the water.

The causes of change in the saltness may be accidental and temporary, or constantly repeating themselves in certain regions. In the first case they produce variable currents, which do not belong to the subject we are considering; but in the second they must confer upon the water a constant tendency to interchange, and call forth constant currents.

In the equatorial regions, for example, the half inch of water evaporated daily leaves constantly its salt behind, which, with the vast depth of the ocean, can hardly add perceptibly to the specific gravity of the rest of the water.

Nevertheless this water, very gradually becoming slightly saltier and heavier, and sinking, must occasion in the depths a current, although a very feeble one, the direction of which must be into the regions where there is little evaporation and considerable atmospheric precipitation, therefore into higher latitudes. Consequently, as we have already seen, the current called forth by the evaporation of the water counteracts that which is produced by the expansion of the water from difference of temperature.

* "On the Composition of Sea-water in different parts of the Ocean," Phil. Trans. Roy. Soc. London, 1865, p. 203.

In the polar sea much water is, in winter, turned into ice, and its salt separated. This salt adds, though only inconsiderably, to the specific gravity of the cold, and hence already heavy, polar water, and contributes a little to the undercurrent in the direction of the equator; so that it acts, contrary to the preceding case, just the same as the difference of temperature. Perhaps it is partly owing to this, that the flow of the Gulf-stream is somewhat stronger in winter than in summer.

In an extremely interesting article on the currents at the southern extremity of America, Mühry*, on the ground of the winter temperature of Patagonia, conjectures that the Brazilian current also is stronger in the winter of the southern hemisphere than in summer.

In summer, when the polar ice melts, a superficial polar current results; for the water from the melting of the ice, having but little saltness, remains at the surface in spite of its coldness. Scoresby remarked that near Spitzbergen the water of the surface was warmer than at some feet depth; and this observation has been recently confirmed by the Swedish Expedition and by the Norwegian Captain Ulve†.

Unfortunately, we do not yet possess any accurate determinations of the greatest density of sea-water at different temperatures and under various pressures. At all events, however, Mühry's view, that sea-water, as well as fresh water, attains its greatest density at $+4^{\circ}\text{C.}$, appears destitute of proof; for it has recently been found that the temperature at very great depths is often below 0°C. It is probable that the great pressure to which the water is there exposed has an influence in preventing it from freezing, even below 0°C. If earlier observations seem to contradict this, the reason may well be that the thermometers were not sufficiently protected against the pressure of the depths, and hence always gave the temperature of the bottom too high. On this defect rests also Ross's well-known theory of a constant temperature of $+39^{\circ}\text{F.}$ at the bottom of the sea.

At the mouths of rivers, the fresh water must spread upon the surface, and therefore give rise to a current running outwards from the mouth, until it is mixed sufficiently with the salt water. To this end, however, the heavy salt water will flow in the opposite direction, toward the mouth—which, even with a slight current, must greatly favour the formation of sandbanks there.

In the case of inland seas where the inflow is greater than the evaporation, as *e. g.* in the Black Sea and the Baltic, just as

* Petermann's *Geographische Mittheilungen*, 1872, vol. xviii. p. 126.

† *Ibid.* p. 317.

with river-mouths, the upper current in the channel of discharge will flow outwards, while an undercurrent of constantly salt water must flow inwards. But in seas where the evaporation exceeds the influx (for example, in the Mediterranean and the Red Sea), the upper current must flow in through the channel, and the undercurrent carry out the superfluous salt. An extremely interesting example of this sort is presented by the Gulf of Karabughaz, which is connected with the Caspian by a very flat channel of only a few feet depth. As the evaporation from the very spacious surface of the gulf, into which no streams flow, is very great, especially in summer, water is perpetually flowing in from the Caspian with a velocity that sometimes rises to 6 knots an hour. Of course this current brings much salt into the gulf, from which it cannot get out again, because the channel is too shallow to permit an outflow beneath. The salt thus accumulating is deposited in crystals on the bottom; and thus the Gulf of Karabughaz plays the part of a saltpan continually withdrawing salt from the Caspian Sea. If in the course of time the sand washed in by the waves should completely block up the shallow strait which joins the gulf to the sea (which would long since have happened if the current were not so strong), Karabughaz as a lake would soon evaporate completely and leave behind a basin of solid salt, such as we see in the Elton-See and in Iletzkaja Saschtschita as formations of primeval times.

If in any part of the ocean animal life be developed in greater abundance than in others, the excessive abstraction of salt or lime from the water by the animals will also give rise to slight movements of the water. Although such movements could scarcely be called a current, it cannot be doubted that such must take place in the deepest seas even at the bottom, because otherwise no life would be possible there; for among the animals living in the depths there are creatures that cannot change their place; nourishment must consequently be brought to them by currents.

The theory of perfect stillness in the bottom waters of the sea* is therefore, like the theories of Ross and Forbes, to be regarded as incorrect.

We have thus examined all the causes which affect the specific gravity of sea-water and the air, and come to the conclusion that differences of temperature and in the saltness of the water of the sea assist only in a slight degree in maintaining the great ocean-currents and the trade-winds, but cannot possibly produce them;

* Mühry says, "At the bottom of the ocean we must assume that there is almost complete stillness."—*Lehre über die Meeres-Strömungen*, p. 5.

to explain the origination of these currents we must search for other forces.

In certain cases, if an already existing current accumulates the heated water in greater quantity, the difference of temperature may indeed accelerate this current; and in the air, if it operates over very considerable spaces, it can generate wind; but in general it is the quickly condensing aqueous vapour which, by the diminution of its tension, plays the chief part in the production of wind.

[To be continued.]

V. *Tidal Retardation of the Earth's Rotation.*

By ROBERT MALLET, F.R.S.*

THE general idea of the retardation of the earth's rotation by the great tide-wave acting as a friction-brake as it progresses under the coercion of the moon, commonly ascribed to Mayer, was, I have good reason to believe, anticipated by Emanuel Kant, though I have not been able myself to verify the passage in his writings. Of the real existence of such a retarding force, however small may be its effect, there can be little doubt since the masterly researches of Adams upon the moon's acceleration. The subject, probably from its inherent complexity, has attracted but little attention, except from astronomical mathematicians; and some points respecting it which have been referred to in more popular works, appear involved in some obscurity. Professor Tyndall, who, in his 'Heat a Mode of Motion,' gives a very lucid popular account of the phenomena (almost, as he states, in the words of Mayer), has in paragraph 697, p. 483 (4th edit.), the following passage:—"Supposing, then, that we turn a mill by the action of the tide, and produce heat by the friction of the millstones; that heat has an origin totally different from the heat produced by another pair of millstones which are turned by a mountain-stream. The former is produced at the expense of the earth's rotation, the latter at the expense of the sun's heat which lifted the mill-stream to its source." This distinction, it seems to me, cannot be maintained. The power of a tide-mill is not derived from the rotation of the earth, nor from the retardation of that rotation by the great tide-wave. The sea, no matter from what cause, rises above its normal level, to which it after a time sinks again. If during the interval we can impound a portion of the mass of water so elevated and let it descend through some machine recipient of water-power, we have the tide-mill, the power of

* Communicated by the Author.

which is as directly derived from gravitation as is that of a water-mill upon a mountain-stream. The water is raised in the former case by gravitation towards the moon and by gravitation falls back towards the earth; in the latter it is raised by evaporation, and falls back to the sea by gravitation. It is true that the earth's and the moon's rotation are "inseparable accidents" to the rise and then the fall of the surface of the sea at any particular point; but the source of the power is derived, not from the mechanism nor at the expense "of the earth's rotation," but as directly from gravitation as is the case in any ordinary mill-stream. If I am wrong in this I shall gladly accept correction. My chief object here, however, is to ask whether (assuming the actuality of retardation of rotation by the tidal wave acting as a brake, and be its amount more or less) there may not be other forces in action upon our globe tending to countervail this to a greater or less extent. It seems to me that there are—though, so far as my reading goes, I have not seen any notice of such on the part of physical writers. Every particle of matter (rotating as part of our earth) which descends from a higher to a lower elevation, must in doing so part with kinetic energy proportionate to its decrease in velocity of rotation between its higher and its lower positions, and the energy so lost is transferred at the lower point to the earth itself. Every drop of water, therefore, every flake of snow that precipitates upon the higher parts of our globe, if assumed to reach these points without relative velocity, must in descending to the sea-level tend to accelerate the earth's rotation. So also every block of ice or of stone that descends from the mountain-tops, every particle of detritus carried along from higher levels towards the sea, must have the same effect. With regard to the first it may be said, every particle of water raised by evaporation from the surface of the ocean ascends into the atmosphere with only a velocity of eastward rotation due to the earth's radius at the sea-level, and at the latitude at which it is taken up, and that therefore when precipitated upon some much higher level it takes away from the earth as much kinetic energy as it returns to it in descending in streams again to the sea-level. But is this so? What actually passes when a particle of sea-water at the surface of the ocean, parting with its salt, rises therefrom under the influence of the sun's heat, and becomes an invisible vapour held in suspension by the air, is to a great extent still unknown. The particle of water, whatever be its physical condition on leaving the liquid surface, undoubtedly only possesses the velocity due to its low position upon the earth's surface; before it has risen even a fraction of an inch, however, it is taken possession of by the air (that is to say, by the winds); and all its

subsequent movements are coerced by them. Except through the winds it has no *point d'appui* upon the solid earth. Now the movements of the winds, however largely modified by the form and rotation of our earth, mainly depend upon differences of temperature produced by the sun's heat; it would seem therefore that, so far as the kinetic energy of the ascending particle of vapour is concerned, it may or may not affect, and, if at all, very slightly, the horizontal motions of the winds, but can have no effect upon the rotation of the earth.

The case is different, however, as soon as the particle of vapour raised by molecular forces to the level of a mountain-top is precipitated thereon as rain or snow, and begins to descend again towards the ocean whence it came: at every foot of its descent it parts with kinetic energy, which it transfers directly to the earth as a whole. On the other hand, such particles of vapour as assumed the form of rain or snow at greater or less elevations, and fall directly as rain-drops to the sea-level, can produce no effect in accelerating the earth's rotation, each drop being coerced in its movements until within a short distance of the earth by the winds—that is, by the same molecular forces which raised them up.

If this speculation be admissible, then we have a source of sensible acceleration to the earth's rotation in the vast volume of water which is precipitated upon the dry land and runs off into the ocean. Adopting Gardner's estimate of the surface of the land, exclusive of the antarctic continent, and assuming a mean annual rainfall for the whole earth of 60 inches per annum, and that two thirds of the entire rainfall returns to the ocean by streams and rivers, we have 23,891 cubic miles of water annually precipitated and falling back into the ocean; and assuming the mean height of the land to be about 1000 feet, this immense mass, on reaching the ocean, has lost kinetic energy due to the difference in velocity of rotation between the earth's mean radius at the sea-level and the same plus 1000 feet, the portion of this which is effective in producing acceleration depending upon the cosine of the latitude.

As respects the descent of solids from higher to lower levels, there seems no room for doubt as to their tendency to produce acceleration in the earth's rotation. It is true that at remote epochs, when continents and mountains were originally elevated, their uplifting tended to retard the earth's rotation, and that their complete ablation could do no more than restore the energy of rotation the earth had before their upheaval. But the ocean-bed was depressed; and its area is four times that of the land, and its mean depth probably greater than the mean height of the continents; if, therefore, we assume the present

ocean-level as a datum plane, the changes of level originating land and sea may have tended rather to accelerate than retard the earth's rotation. Taking the mean of the sediment stated to be carried by six great rivers, namely the Mississippi, Po, Vistula, Rhine, Ganges, and Rhone, it amounts to about $\frac{1}{1200}$ in volume of the water discharged; and if we apply this to the water discharged from the whole surface of the land, as above stated, we have $\frac{23891}{1200} = 19.90$ cubic miles of sediment annually discharged at the sea-level. These rough estimates are probably far from correct, and we do not know with any precision what is the mean specific gravity of this sediment, nor from what mean height it may be considered to have descended; but we can easily see that the loss of rotative energy during the descent of this vast mass, if transferred to the globe as a whole, is scarcely negligible. Nor does this represent all that we have to deal with. The sediment finally carried into the sea represents the real annual degradation of the land by rain and rivers; and the huge block that falls to-day from a Sierra summit and wedges itself a few miles off immovably in the cleft of a cañon, though it may not reach the sea for thousands of years, during which it is slowly transformed into sediment, is nevertheless effective, as is the ice which thaws or falls in avalanche, in transferring to our earth the energy of rotation they lose in descent. Whether or not it be true that, viewed on its largest scale and at some indefinitely remote period yet to come, the movements of all the bodies of the universe tend to ultimate rest, and an end of the present order of things, it seems a fact that all the smaller perturbations of planetary movement at least, as for example those of precession and nutation, are involved in conditions which prevent their passing a certain limit, and which in other cases equilibrate the disturbing cause. It would seem, therefore, contrary to analogy to suppose the case of the retardation of our globe by tidal friction, whatever may be its actual amount, to be an exception and to go on unchecked, until the astronomical consequences pointed out by Thomson and others shall have occurred in the motions of our satellite, our earth, and the sun.

VI. *On some points in Mallet's Theory of Vulcanicity.*

By EUG. W. HILGARD, *University of Michigan*.*

THE main points of Mallet's Theory of Vulcanicity have been before the world of science for some time, and have excited some lively discussions on both sides of the question,

* From Silliman's American Journal, June 1874.

mainly in the English press. I think it is to be greatly regretted that the original memoir, very tardily published in the Transactions of the Royal Society, should be so difficult of access, that few of those interested are enabled to appreciate the caution and laborious conscientiousness which Mallet has brought to bear on his investigation and discussion of this most complex problem, and to what extent he has himself anticipated most of the objections raised. In calling attention to some apparent omissions in this respect, it may be useful to recall the state of the question as regards some of the more prominent points at issue.

The first and most sweeping attack upon the very basis of Mallet's theory comes from Sir William Thomson, in a letter to Mr. Poulett Scrope (*Nature*, Feb. 1, 1872), in which he calls attention to, and reaffirms the results of his investigation (supplementary to that of Hopkins) on the effect which a fluid nucleus and imperfect rigidity of the earth must exert upon precession and nutation, and which led him to the conclusion that, unless the rigidity of the globe as a whole were greater than that of steel, there must ensue a tidal deformation of the solid mass, which would sensibly change the amount of precession. He denies that Delaunay has shaken, in any important point, the conclusions of Hopkins or himself.

The subject has since been taken up by General Burnard (*Smith's Contr.* No. 240), who, while confirming the results of Thomson upon the premises assumed by that physicist, also shows that there are assumable and admissible conditions upon which a fluid nucleus with a moderately thick crust may exhibit the same constant, or periodically recurrent, amounts of precession and nutation as a solid globe.

Mallet refers to Thomson's argument in favour of great rigidity as corroborative of the necessity for assuming a crust of great thickness, such as would render it inadmissible to assume a direct connexion between volcanoes and the liquid nucleus. But it is difficult to see how the "preternatural rigidity," made a postulate by Thomson, could in any manner be compatible with the requirements of Mallet's theory. For the latter represents the earth's crust as a congeries of fragments, sustained partly by the contracting liquid nucleus, partly by each other on the principle of the arch—therefore necessarily often locally in a state of unstable equilibrium, and liable to be disturbed by slight outside forces. That the tendency to tidal deformation contributes toward producing such disturbances has been rendered probable by Perrey's discussions, and by the repeated coincidence of violent earthquakes with tidal extremes, lately observed.

Thomson's assumption, that the postulated rigidity might

result from compression, would scarcely seem admissible, save in a case of *absolute* homogeneity and equilibrium—if then. It is certainly incompatible with the demonstration made by Professor Belli of Pavia (as quoted by Mallet), to the effect that rigid bodies are weakened by the simultaneous application of orthogonal pressures—that no known materials could sustain, under any circumstances, a strain several hundred times greater than that which would crush it if laterally free to yield—that such strains exist in the contracting crust, and that upward deformation must result, if such contraction takes place at all, as the annual loss of heat by the earth compels us to assume is the case.

Whether we view the question of rigidity by the light of our direct knowledge of the first twenty-five miles of crust, and of the profound commotions it experiences from time to time, or by that of the demonstrated increase of temperature as we descend, rendering it extremely probable that at a comparatively slight depth the rigidity of all materials must be seriously impaired by a high temperature despite of pressure—or whether we even consider alone the secular loss of heat by radiation, which must result in a contraction affecting unequally the heterogeneous *couches* of which, on any hypothesis, the solid portion of the earth must be composed—it will be difficult to persuade geologists of the actual existence of the “preternatural rigidity” until every reasonable hypothesis that can dispense with this assumption shall have been exhausted.

Among the objections raised by geologists, the first, and apparently gravest, was that of Forbes (*Nature*, Feb. 6, 1872), who argues the untenableness of Mallet's theory on the ground of the asserted general identity of composition of volcanic ejecta. In fact, from Mallet's point of view, it would seem that lavas might have the composition of any fusible rock whatsoever in whose strata the crushing might happen to occur, and hence that, if taking place within the sedimentary strata, there ought to be a very great diversity between the ejecta of different vents.

In his rejoinder Mallet calls attention to the very serious differences of composition between the extremes of trachytic and basaltic lavas, and to the generally admitted fact that volcanoes are located along axes of upheaval, where the hypogene rocks, and therefore those of the crust proper, approach the surface—hence that crushing along these lines of weakness would be by no means likely to produce a greater diversity of lavas than we actually observe. Furthermore, that the “local lake” theory is liable to the same objection, unless the lakes are supposed to be located within the (uniform) crust itself.

He might, it seems to me, have added that the maximum of

twenty-five miles of sedimentary rocks is not anywhere (on the continental areas at least) actually superimposed vertically upon the crust, and hence that it is not unreasonable to assume that a pressure sufficiently great to produce fusion may never occur within the limits of the sedimentary strata, albeit other manifestations of subterraneous thermal action may not be wanting. It is true that, on the whole, Mallet's memoir leaves upon the reader's mind the impression that he seeks the source of volcanic action at depths sufficiently shallow to justify in a measure the objection raised by Forbes, although he expressly declares that, with our present data, the determination of the points at which the maximum of crushing-effects occurs is impossible.

Similar considerations apply to the objection raised by F. W. Hutton (*Nature*, Nov. 27, 1873), that "faults show no heating-effects, even where considerable crushing has taken place." The pressure under which the faulting occurred may have been inadequate, in the cases coming under our observation; but above all, *time* is a most essential element in this connexion. No matter how great the dislocation or crushing, no great increase of *temperature* can occur if it takes place *slowly*, however great may be the *quantity* of work performed, or of heat produced. And very many, if not the majority of extensive faults actually occurring, show evidence of having been formed without cataclysmal disturbance.

Among the other points raised by Hutton (*loc. cit.*) there are several which are at once disposed of by a perusal of the original memoir. There are others of some weight. That "lines of least resistance once chosen must remain," is doubtless true in a very wide sense; and in that sense this is scarcely at variance with observed facts, since the lines of weakness along the borders of continents are still those which exhibit volcanic activity (and earthquake phenomena) most frequently. But in the folding and upheaving of strata by tangential thrust the question of equilibrium must often of necessity be very delicately balanced, depending as it does upon the vertical pressure of the masses, their nature, dislocation, subsequent consolidation, igneous effusions from fissures, &c. Lines of weakness as to *rigidity* may thus easily acquire sufficient *static* resistance to cause a subsequent yielding to take place at some distance from the original axis, as is exemplified in the formation of successive parallel ranges. What is true with regard to the formation of folds is equally so as concerns the settling down of the crust-fragments in consequence of interior contraction. Each fragment as a whole may remain as such, being only, as it were, abraded at its circumference. But it is only necessary to have

observed the gradual yielding of detrital rock-masses under pressure, to understand why the cataclysmal yielding which manifests itself in earthquakes should so frequently change its locality of occurrence—why for long periods a region may be completely exempt from these movements, in consequence either of an unresisted and therefore gradual descent of the crust-fragments underlying it, or of an arch-like arrangement, whose sudden breaking down will result in a catastrophe, succeeded perhaps by a long period of quiescence.

Thus Mallet's theory accounts equally well for the sporadic and apparently lawless occurrence of seismic phenomena, and for the probable correlation between the frequency and violence of earthquakes and tidal extremes. Unlike the theory of a thin crust, which would lead us to expect almost diurnal earthquakes corresponding to oceanic tides, according to Mallet's view there should be a near coincidence in time and space of two independent factors (viz. of a condition of very unstable equilibrium of some crust-fragment, with a tidal extreme) in order to produce a maximum of disturbance. It cannot be expected that such coincidence should be of frequent occurrence, or that the casual connexion should manifest itself in a greater predominance than that claimed by Perrey for the times of spring and neap tides. Mallet does not, however, allude to this point—whether from a distrust of Perrey's data and method, or theoretical scruples on the score of "rigidity."

The objection, that according to Mallet's theory earthquakes ought always to be followed by eruptions, could obviously apply only during the period of fissure eruptions from the liquid interior—it being conceded that the volcanic eruptions of to-day are due to contact of water with the molten rock, and that steam, not static pressure, is the *vis a tergo*. It is, of course, very probable that the access of water to the volcanic focus* is generally caused or facilitated by such crust-movements as would at the same time result in the production of more heat and perhaps of fused rock, such movements being indicated by the (mostly slight) earthquakes that so frequently precede a period of volcanic activity. Hutton's objection, that according to Mallet's view each eruption ought to be preceded by a sensible subsidence, is therefore groundless.

One point, however, must strike every reader of the original memoir, viz. the preeminence given by Mallet to the *crushing of solid rock* as the means of producing heat and fusion. One would naturally look to the results of his experiments on this

* Hutton (*loc. cit.*) avers that "to cause a volcano the heat must go to the water; the water cannot go to the heat," but omits any explanation of this singular axiom.

subject for the proof of the efficiency of this agency. But we find that the maximum of *temperature* resulting from the crushing to powder* of the hardest rock is something over 217° Fabr. This, then, represents the maximum increment of temperature that can be rendered efficient toward the fusing of rocks by the crushing process under the most favourable circumstances, viz. upon the supposition that it takes place instantaneously, or under such circumstances that the heat cannot be conducted away, and, further, that the resistance of the rock has not been materially diminished by the downward increase of hypogeal temperature. At the most moderate depths at which volcanic phenomena can be supposed to originate, the last-mentioned factor must exert a very considerable influence, reducing materially the available heat-increment. Hence the numerical results of Mallet's laborious experiments on rock-crushing, however interesting and useful as affording a definite measure of the thermal effects producible by this means, yet fail to carry conviction as to the efficacy of this particular *modus operandi* in reducing large masses of solid rock to fusion, unless essentially supplemented by *friction*, not so much of rock walls against each other, but more probably by the heat produced within more or less comminuted *detrital* or *igneoplastic* masses by violent pressure and deformation.

It may be doubtful what would be the physical and thermal effect of enormously great pressures upon rock powder such as was produced in Mallet's experiments; but it would seem that if *made* to yield, the frictional effect must produce very high temperatures. *A fortiori*, solid detrital masses of variously sized fragments intermingled (such as, rather than powder, would be likely to result from steady pressure), yielding rapidly under great pressures, might, under the *combined influence of friction and rock-crushing*, well be supposed to reach the temperature of fusion, which a simple crushing of a solid mass by pressure would have failed to produce. Mallet mentions the probable influence of friction, and of the squeezing of igneoplastic masses, but does not attach to these agencies such importance as they seem to me to deserve.

Of the complex thermal effects of the movements of detrital masses under great pressure, Mallet's figures of course offer no measure whatsoever; nor is this, or even the thermal coefficients resulting from his rock-crushing experiments, at all necessary to the establishment of the postulates of his theory.

* Mallet does not go into the consideration of the physical nature of this "powder," and of the thermal and other differences likely to result from its production under pressures enormously greater than those employed by him.

Taking for granted the correctness of Hirn's theorem, "that the heat evolved in the crushing of rigid bodies is the equivalent of the work performed," Mallet's experiments on the contraction of fused rock in cooling, and his estimates of the amount of volcanic energy manifested on the globe, coupled with that of the earth's annual loss of heat, completed the proof of the *quantitative* adequacy of the cause invoked by him. And when it is understood that the earth's present loss of heat during sixteen and a half years is the mechanical equivalent of all the volcanic work performed since the period of fissure eruptions, the burthen of proof of the *qualitative* inefficiency of the several modes of action that may come into play would seem to be effectually thrown upon the opponents of the theory.

Among these modes of action, the fusion of masses already existing in a pasty, or generally more or less igneoplastic condition, by squeezing or forcible displacement, seems to me to deserve especial attention. At the depth at which volcanic phenomena must be supposed to originate, this condition must be closely approached, especially in the early times of the volcanic period—that of the "Maare" of the Eifel and other similar cases representing the transition phase between the régime of fissure-eruptions and that of volcanoes proper. In this period of a "greatly stiffened and thickened crust," even slight flexures, whether synclinal or anticlinal, would occasion great displacements and movements in the half-stiffened upper layers of the "viscous *couche*;" and if these experienced local refusal, the fused matter may well be presumed to have often been disposed of by eruption through fissures or volcanic vents, rather than by overcoming downward the inertia of the viscous *couches*. This mode of action seems to me likely not only to afford a more copious, but also a more constant or lasting source of supply than the supposed crushing of solid rock, and appears especially applicable to the case of large fissure-eruptions.

Among the greatest services rendered by Mallet's (or, in this connexion, Wurtz's) theory is the unstrained explanation of many of the phenomena of metamorphism that were quite unintelligible so long as the heat required for the observed changes was supposed to be derived from below, and perhaps by transmission through strata which themselves had experienced little or no change of condition. The principle that the heat evolved in the flexure or forcible compression of strata is, *cæteris paribus*, proportional to the resistance offered by them to the external force, throws a flood of light upon numerous apparently contradictory phenomena, which have long been quoted as incompatible with the doctrine of metamorphism as held in this country, and have stood in the way of its general

acceptance by geologists, particularly on the continent of Europe. In its application to the formation of synclinoria especially, the principle works most instructively and satisfactorily. It can scarcely be doubted that in the first folding of the vertex of a geosynclinal, weakened below by fusing away and heating of the crust and lowest strata, the movements were comparatively localized and rapid, and therefore capable of producing high temperatures, and their results such as we now usually find them along the main axes of elevation of synclinoria. But as the resistance along this axis increased by emergence and solidification, the points of yielding (*i. e.* the folds) would be *multiplied*, while the absolute amount of motion transformable into heat would be *diminished* in each. Hence the decrease *in general* of metamorphic effects as we recede from the main axis. And yet it is perfectly easy to conceive of large local exceptions to the general rule (such as we actually observe), on the basis of greater resistance in perhaps a localized stratum of a lateral fold, yet so situated that it could not successfully resist the influence of an advantage of leverage causing a rapid deformation. It is even predicable that under such circumstances sudden breaks and crushings must occasionally have occurred, giving rise to fusion of rocks and limited fissure-eruptions, or at least to pasty rock intrusions—as suggested by Dana for granitic and analogous veins, that show no evidences of the cooperation of very high temperature in the act of formation.

LeConte's view, that the first mashing of a geosynclinal would produce *less* heat than later plications*, in which (presumably) a greater resistance would have to be overcome, seems hardly to be compatible with facts as generally observed away from the Pacific-coast eruptions; and his argument is the less cogent, as the temperature produced is a function, not only of the resistance of the rocks, but also of the *degree* and *rapidity* of the motions, both of which have been on the decrease in late geological periods, in accordance with the diminishing rate of contraction of the earth and the increased resistance of the crust to flexure.

While Mallet's theory accounts satisfactorily for earthquake phenomena and volcanic activity as manifested since the cessation of fissure-eruptions, and also for the gradual or sudden *depression* of both large and small areas even subsequent to that time, it makes no provision for their *elevation*, and therefore leaves unexplained the numerous *oscillations* of level of which we find the record down to our own time. In assuming

* "On the great Lava-flood of the West," Silliman's American Journal, March 1874, p. 179.

the movements as taking place exclusively within the solid shell, he (unnecessarily as it seems to me) leaves a point open to objection.

While admitting that slow secular oscillations, or those minor changes of level constantly occurring in volcanic areas, may even now in many cases be reasonably attributed to changes of temperature occurring within the solid rocks themselves, and within their limits of elasticity, it is impossible to assign this as an adequate cause of those extensive oscillations which have characterized the Quaternary period, and are recorded, *e. g.*, by the raised beaches of the North-Atlantic coasts and inlets, and by the drift-pebbles even now found four hundred and fifty feet below the level of the Gulf of Mexico, while the emerged formations record a complementary elevation to at least a similar extent during the Terrace epoch. This record of an oscillation of near a thousand feet on the Gulf-shore since the glacial-drift epoch, implies *at least* a corresponding one over the greater portion of the area drained by the Mississippi, unless that river flowed backward at one time*. Doubtless these oscillations, like the glaciation of which they probably were cooperative causes, were of continental extent, as was the (more or less contemporary) emergence of the Siberian plain; and as such they must be presumed to have been true movements of the earth's crust, although lying quite within the volcanic period proper. It is but reasonable to suppose that the sinking of the great Pacific area was then, and may still be, of a similar nature.

If Mallet's theory, as well as the geological facts with which it deals, is incompatible with Hopkins's and Thomson's postulate of extreme rigidity; if, as it appears to me, the events of very recent geological epochs in connexion with the very slow rate of cooling since that time render it unlikely that the crust can even now be considered rigid in a geological sense; if, finally, as General Barnard affirms, the astronomical objection to a comparatively pliant crust and liquid nucleus is not absolute,

* It is a curious fact that in the various hypotheses regarding the oscillations of the continental interior during the Drift epoch, the facts observed on the Gulf-shore have over and again been quietly ignored, although the Gulf is unequivocally the natural reference-level most directly related to that interior, not only at the present time, but, as the direction of the Drift currents and the trend of the formations show, ever since the time of the Cretaceous emergence. Nevertheless the reference-level has been sought beyond the Alleghany upheavals, or beyond the fixed Azoic area upon which the movement appears, in a measure, to have pivoted, and where, as Dana has shown, it was materially diminished in extent. Assuredly no hypothesis which disregards the changes of level registered at the continental outlet has any *raison d'être*!

but may be obviated by admissible assumptions regarding the mode of distribution of the solid and liquid matter constituting the globe,—we are led to the reasonable assumption that while the thickness and rigidity of the crust is evidently too great to admit of further folding or fissure-eruptions, and (probably) to admit of connecting ordinary volcanic phenomena directly with the (virtually or actually) liquid interior, yet we need not assume it to be so great as to render the crust incapable of yielding somewhat, *on a large scale*, to static upward pressure. Such pressure may be either the resultant of tangential stress, such as might slightly deform an arch without fracture or folding, or even the direct result of a corresponding subsidence elsewhere.

The latter effect would of course be incompatible with a shrinking away of the fluid interior from the crust, as required by Mallet's theory, if it were necessary to assume that the interior crust-surface is substantially "smooth," *i. e.* free from important downward projections or upward sinuosities. But so far from this, the cooling influence that has so long acted on the oceanic areas, contrasted with those enormous outwellings of igneous rock that have occurred even in late Tertiary or Posttertiary times, together with other considerations, necessitate the assumption that such inequalities do exist to a notable extent. Hence the overlapping alluded to by Mallet of the period of fissure-eruptions and of that of volcanic activity proper, which appear to have coexisted, in *different* portions of the globe, from early Tertiary to early Quaternary times. For even Mallet himself considers the outpourings of igneous rocks on the Pacific coast "wholly inconsistent with existing volcanic forces;" and few geologists will agree with LeConte* in ascribing precisely these most extensive fissure-eruptions in the world to the "ineffectual fires" of the volcanic period, arising alone from transformed motion.

Indeed it is not easy to understand the precise mechanism of the great fissure-eruptions as a consequence of nucleal contraction, without the aid of some static head of pressure that may exist more or less locally, in consequence of inequalities in the crust (whether of form, thickness, or density), and thus act as *a vis a tergo*.

At first blush the "squeezing out of sub-mountain liquid matter," assumed by LeConte as the consequence of the folding and fissuring of strata by tangential thrust, appears natural enough. Yet it seems hardly possible that the same force which makes and *elevates* mountain folds (being the *result of interior shrinkage*) should at the same time serve to *compress the*

* Silliman's American Journal, March 1874, p. 179.

interior liquid, unless either such folding occurs beneath the general level of the liquid, or the latter is locally confined, or the movement is so (comparatively) brusque or cataclysmal, that viscosity would prevent the lateral or downward escape of the liquid rock. In the case of the Pacific eruptions the evidence of steady static outflow and regular upbuilding is especially cogent; and, as LeConte remarks, it has been slow work, as indeed is usually or universally the case with mountain-building*.

The assumption of locally limited fire seas with a solid globe as made by Dana† in conformity with Hopkins's views, would remove the difficulty if the crust could be assumed as contracting on the whole independently of the portions over fire seas. But when we come to discuss the application in detail of this intrinsically improbable hypothesis, we find the required extent and localities of these fire seas to be such that we can hardly imagine them to be effectually separated from each other; in other words, we approach very near to a condition of general undercrust fluidity up to late geological periods‡. It then becomes a question of minor importance whether there is a central nucleus solidified by pressure, or whether all within the crust is actually liquid.

The inherent improbability of the depression of a geosynclinal trough to a level so low as to allow the liquid rock to *rise into it*, as it were, is too great to render its discussion necessary.

Indeed it seems almost impossible to imagine a mechanism explaining satisfactorily fissure-eruptions such as those of the Pacific coast, on the basis of a slowly contracting *solid* crust with a rapidly contracting *liquid* layer or nucleus beneath. A more satisfactory explanation seems possible if, in accordance with Mallet's suggestion and the intrinsic probabilities of the

* When LeConte says (*loc. cit.* p. 179) that the out squeezing of the liquid has been caused by "enormous horizontal pressure, determined by the interior contraction of the *whole* earth," and then (p. 180) that, "whether by uplifting or upbuilding, the actual increase of height would be precisely the same, being determined by the amount of lateral crushing," he seems to think of crust-contraction upon a nucleus *too large* for it, rather than of Mallet's "freely descending" crust. Or, if he considers the fused rock the result of motion transformed, it is difficult to see on what ground a simple "*uplifting*" could be considered the precise mechanical equivalent of an *upbuilding* by eruption of liquid rock. In either case the *lifting* done would be the same; but what of the enormous *heat of fusion*?

† "On some of the Results of the Earth's Contraction," Silliman's American Journal, August 1873, p. 105.

‡ Ibid. July 1873, p. 7 *et seqq.*

case, we assume the existence of a thickly viscid, igneoplastic undercrust layer. Such a layer, while barely or very slowly obeying the laws of liquid equilibrium, would be capable of being liquefied by a slight increase of temperature, such as might be produced by squeezing or kneading. Portions of such plastic matter would occasionally become involved in the anticlinal folds of synclinoria, and thus supply the material for limited fissure-eruptions, in that case literally "squeezed out." But the inverse ratio pointed out by Dana as existing between folding and fissure-eruptions points to the rarity of such events.

At any rate they could not explain the outwellings of the Pacific border, which continued long after close plications had ceased to be made—in fact, as it would seem, up to the end of the period of elevation of the main Sierra Nevada.

It is but fair to assume that near lines of weakness indicated by plications or fissure-eruptions, the isogeotherms *have been* during the elevation of mountain-chains (and probably still *are* where such lines are marked by volcanic vents) considerably above their general level. In an anticlinal upheaval they would probably conform to the progress of the sublevatory movement, in a ratio more or less directly proportional to the rapidity of the upward movement, and would gradually descend during periods of repose. This would happen independently of any heat generated by transformation of motion.

In a polygenetic chain like the Sierra Nevada, after the collapse and folding of the geosynclinal and the subsequent stiffening of the backbone (so to speak), any further elevation of the *main ridge* becomes a *quasi-anticlinal* movement, accompanied necessarily by the compression and "squeezing" of the heated rocks embraced within the arch. The heating being greatest, *ceteris paribus*, where the resistance and motion is a maximum, more heat would be generated by the compression of the upper, half-stiffened portion of the viscous or igneoplastic layer, than in the lower ones; and the liquid matter so formed would constitute a head of pressure, from which fissure-eruptions might derive their material; whether directly, or by pressure communicated to more distant points of rupture and fusion by lateral stress.

If, then, as LeConte's data seem to show, the final and most considerable anticlinal elevation of the great interior range took place during the same period that witnessed the great fissure-eruptions of the Coast and Cascade ranges, it may not be unreasonable to suppose these events to have not only been contemporaneous, but to have borne to each other something of the relation of cause and effect, and that each of the numerous

superimposed strata of igneous rock in the latter region may represent not only the direct effect *in loco* of more or less paroxysmal thrusts, but also the reflex action of the simultaneously progressing anticlinals in the high Sierras.

VII. *A New Formula in Definite Integrals.*

By J. W. L. GLAISHER, M.A.*.

1. **I**NTEGRATE the identity

$$a_0 - a_1 x^2 + a_2 x^4 - \dots = \frac{a_0}{1+x^2} - \Delta a_0 \frac{x^2}{(1+x^2)^2} + \Delta^2 a_0 \frac{x^4}{(1+x^2)^3} - \dots$$

(where $\Delta a_n = a_{n+1} - a_n$) between the limits zero and infinity, and the right-hand side becomes

$$\begin{aligned} & \int_0^\infty \left(\frac{a_0}{1+x^2} - \Delta a_0 \frac{x^2}{(1+x^2)^2} + \Delta^2 a_0 \frac{x^4}{(1+x^2)^3} - \dots \right) dx \\ &= \int_0^{\frac{\pi}{2}} (a_0 \cos^2 \theta - \Delta a_0 \tan^2 \theta \cos^4 \theta + \dots) \sec^2 \theta d\theta \\ &= \int_0^{\frac{\pi}{2}} (a_0 - \Delta a_0 \sin^2 \theta + \Delta^2 a_0 \sin^4 \theta - \dots) d\theta \\ &= \frac{\pi}{2} \left(1 - \frac{1}{2} \Delta + \frac{3}{4} \cdot \frac{1}{2} \Delta^2 - \frac{5}{6} \cdot \frac{3}{4} \cdot \frac{1}{2} \Delta^3 + \dots \right) a_0 \\ &= \frac{\pi}{2} (1 + \Delta)^{-\frac{1}{2}} a_0 = \frac{\pi}{2} E^{-\frac{1}{2}} a_0 = \frac{\pi}{2} a_{-\frac{1}{2}}; \end{aligned}$$

so that

$$\int_0^\infty (a_0 - a_1 x^2 + a_2 x^4 - \dots) dx = \frac{\pi}{2} a_{-\frac{1}{2}} \dots \dots \dots (2)$$

The definition of the symbol E is contained in $Ea_n = a_{n+1}$; and of course, a_n being only defined for n a positive integer, $a_{-\frac{1}{2}}$ is without meaning. But in cases where a_n involves factorials, there is a strong presumption, derived from experience in similar questions, that the formula will give correct results if the continuity of the terms is preserved by the substitution of gamma functions for the factorials. This I have found to be true in every case to which I have applied (2).

* Communicated by the Author.

E. g. (i) Let

$$a_n = \frac{a^{2n+1}}{1 \cdot 2 \dots (2n+1)} = \frac{a^{2n+1}}{\Gamma(2n+2)};$$

then $a_{-\frac{1}{2}} = 1$, and

$$\int_0^\infty \frac{\sin ax}{x} dx = \frac{\pi}{2},$$

which is true.

(ii) Let

$$a_n = \frac{a^n}{1 \cdot 2 \dots n} = \frac{a^n}{\Gamma(n+1)},$$

then

$$a_{-\frac{1}{2}} = \frac{a^{-\frac{1}{2}}}{\Gamma(\frac{1}{2})} = \frac{1}{\sqrt{\pi a}},$$

and

$$\int_0^\infty e^{-ax^2} dx = \frac{\sqrt{\pi}}{2\sqrt{a}},$$

the true result.

If we take $a_n = \frac{a^{2n}}{1 \cdot 2 \dots 2n} = \frac{a^{2n}}{\Gamma(2n+1)}$, we have

$$\int_0^\infty \cos ax dx = 0,$$

viz. $\sin \infty = 0$, the value we should expect to find by any process that gave a result at all.

2. Divide (1) by $1+x^2$ and integrate as before: the right-hand side

$$\begin{aligned} &= \int_0^{\frac{\pi}{2}} (a_0 \cos^2 \theta - \Delta a_0 \sin^2 \theta \cos^2 \theta + \Delta^2 a_0 \sin^4 \theta \cos^2 \theta - \dots) d\theta \\ &= \frac{\pi}{2} \left(\frac{1}{2} - \frac{1}{4} \cdot \frac{1}{2} \Delta + \frac{3}{6} \cdot \frac{1}{4} \cdot \frac{1}{2} \Delta^2 - \frac{5}{8} \cdot \frac{3}{6} \cdot \frac{1}{4} \cdot \frac{1}{2} \Delta^3 + \dots \right) a_0 \\ &= \frac{\pi}{2} \frac{\sqrt{1+\Delta}-1}{\Delta} a_0 = \frac{\pi}{2} \frac{1}{1+\sqrt{E}} a_0; \end{aligned}$$

so that

$$\int_0^\infty \frac{a_0 - a_1 x^2 + a_2 x^4 - \dots}{1+x^2} dx = \frac{\pi}{2} (a_0 - a_{\frac{1}{2}} + a - a_{\frac{3}{2}} + \dots). \quad (3)$$

Take

$$a_n = \frac{a^{2n}}{1 \cdot 2 \dots 2n} = \frac{a^{2n}}{\Gamma(2n+1)},$$

and

$$\int_0^\infty \frac{\cos ax}{1+x^2} dx = \frac{\pi}{2} \left(1 - a + \frac{a^2}{1 \cdot 2} - \dots \right) = \frac{\pi}{2} e^{-a}. \quad (4)$$

Similarly, by taking $a_n = \frac{a^{2n-1}}{\Gamma(2n)}$, we obtain the correct value of $\int_0^\infty \frac{x \sin ax}{1+x^2} dx$.

The peculiarity of (2) and (3) consists in the appearance on the right-hand side of terms with fractional arguments. In such an equation as (4), where one side is a function of a^2 , while the other involves uneven powers of a , it seems as though it would be impossible to evaluate the integral by any direct procedure; for *à priori* it would appear that no method of expansion and integration term by term could transform a function of a^2 into one of a , and thus, as it were, extract the square root of a constant involved. The way in which the symbolic process introduces \sqrt{E} , and so actually does effect this conversion, is interesting: when I first applied the identity (1) to the integral in (4), I scarcely expected to obtain any result capable of interpretation.

Whenever (2) and (3) admit of interpretation, it is highly probable that the result so given will be the true one; *e. g.*, taking

$$a_n = \frac{a^n}{\Gamma(n+1)}, \text{ we find}$$

$$\begin{aligned} \int_0^\infty \frac{e^{-ax^2}}{1+x^2} dx &= \frac{\pi}{2} \left\{ \frac{1}{\Gamma(1)} - \frac{a^{\frac{1}{2}}}{\Gamma(\frac{3}{2})} + \frac{a}{\Gamma(2)} - \frac{a^{\frac{3}{2}}}{\Gamma(\frac{5}{2})} + \dots \right\} \\ &= \frac{\pi}{2} \left\{ 1 - \frac{2a^{\frac{1}{2}}}{\sqrt{\pi}} + a - \frac{2a^{\frac{3}{2}}}{1 \cdot 3 \cdot \sqrt{\pi}} + \frac{a^2}{1 \cdot 2} - \dots \right\} \\ &= \frac{\pi}{2} \left\{ e^a - \frac{2a^{\frac{1}{2}}}{\sqrt{\pi}} \left(1 + \frac{2a}{1 \cdot 3} + \frac{(2a)^2}{1 \cdot 3 \cdot 5} + \dots \right) \right\} \\ &= \sqrt{\pi} \cdot e^a \left\{ \frac{\sqrt{\pi}}{2} - \int_0^{\sqrt{a}} e^{-x^2} dx \right\} = \sqrt{\pi} \cdot e^a \int_{\sqrt{a}}^\infty e^{-x^2} dx, \end{aligned}$$

the known value. But (2) and (3), as general formulæ, are remarkable; and they would give results in very many cases where it might not be easy to evaluate the integrals otherwise.

Trinity College, Cambridge,

June 19, 1874.

VIII. *On some Physical Properties of Ice ; on the Transposition of Boulders from below to above the Ice ; and on Mammoth-remains.* By JOHN RAE, M.D., LL.D., &c.*.

IS the ice formed on salt water fresh ? or, in other words, if ice formed on the sea is thawed, will the water obtained thereby be fresh ?

For a number of years past I have spoken with many persons on the above subject ; and seldom, if ever, have I found a single individual who did not say that the ice of the sea was fresh.

Some of these gentlemen are known in the scientific world ; and many of them supported their opinions by quoting the highest written authorities on the subject, chiefly Tyndall's 'Forms of Water,' p. 132, par. 339, which tells us that "even when water is saturated with salt, the crystallizing force studiously rejects the salt, and devotes itself to the congelation of the water alone. *Hence the ice of sea-water, when melted, produces fresh water.*"

It is the sentence in italics to which I wish to draw particular attention.

It would be the extreme of folly and presumption on my part to question the correctness of results obtained by scientific men in their experiments in freezing small quantities of sea-water by artificial means, more especially those of the distinguished gentleman whose name I have mentioned, who, in addition to holding the high position of being one of our greatest authorities in all that relates to physical science, possesses the rare gift of being able to communicate his knowledge in such plain, clear, and forcible language, illustrated by admirable experiments, as to make his meaning fully understood, even by those who had previously been perfectly ignorant of the subject.

It is only where I have had opportunities of witnessing the action of cold carried on in a manner which may have been denied to the scientific man, that I venture to differ from him ; and it is in this way that the conviction has been forced upon me, that the ice of sea-water if melted *does not* produce fresh water.

Before entering upon this subject, however, let me say a word or two on the first part of the quotation I have given.

If a saturated solution of salt is frozen, and the ice so formed is fresh, it is evident that the salt that has been "rejected" must be deposited or precipitated in a crystalline or some other solid form, because the water, if any, that remains unfrozen,

* Read before the Physical Society, May 9, 1874. Communicated by the Society.

being already saturated, can hold in solution no more salt than it already contains.

Could not salt be obtained readily and cheaply by this means from sea-water in cold climates?

During several long journeys on the Arctic coast, in the early spring before any thaw had taken place, the only water to be obtained was by melting snow or ice. By experience I found that a kettleful of water could be obtained by thawing ice with a much less expenditure of fuel, and in a shorter time, than was required to obtain a similar quantity of water by thawing snow. Now, as we had to carry our fuel with us, this saving of fuel and of time was an important consideration, and we always endeavoured to get ice for this purpose. We had another inducement to test the sea-ice frequently as to its freshness or the reverse.

I presume that almost every one knows that to eat snow when it is very cold, tends to increase thirst, whereas a piece of ice in the mouth is refreshing and beneficial, however cold it may be; we were consequently always glad to get a bit of fresh ice whilst at the laborious work of hauling our heavy sledges; yet with these strong inducements we were never able to find sea-ice, *in situ**, either eatable when solid or drinkable when thawed, it being invariably much too salt. The only exception (if it may be called one) to this rule, was when we found rough ice, which, from its wasted appearance and irregular form, had evidently been the formation of a previous winter. This old ice, if projecting a foot or two above the water-level, was almost invariably fresh, and, when thawed, gave excellent drinking-water. It may be said that these pieces of fresh ice were fragments of glaciers or icebergs; but this could not be so, as they were found where neither glaciers nor icebergs are ever seen.

How is this to be accounted for? Unfortunately I have only a theory to offer in explanation.

When the sea freezes by the abstraction of heat from its surface, I do not think that the saline matter, although retained in and incorporated with the ice, assumes the solid state, unless the cold is very intense, but that it remains fluid in the form of a very strong brine enclosed in very minute cells. So long as the ice continues to float at the same level, or nearly the same level, as the sea, this brine remains; but when the ice is raised a little above the water-level, the brine, by its greater specific gravity, and probably by some solvent quality acting on the ice, gradually drains off from the ice so raised; and the small cells,

* What I mean by ice *in situ* is ice lying flat and unbroken on the sea, as formed during the winter it is formed in.

by connecting one with another downwards, become channels of drainage.

There may be several other requisites for this change of salt ice into fresh, such as temperature raised to the freezing-point, so as to enable the brine to *work out* the cell-walls into channels or tubes—that is, if my theory has any foundation in fact, which may be easily tested by any expedition passing one or more winters on the Arctic, or by any one living where ice of considerable thickness is formed on the sea, such as some parts of Norway.

All that is required, as soon as the winter has advanced far enough for the purpose, is to cut out a block of sea-ice (taking care not to be near the outflow of any fresh-water stream) about 3 feet square, remove it from the sea to some convenient position, test its saltness at the time, and at intervals repeat the testing both on its upper and lower surfaces, and observe the drainage if any.

The result of the above experiment, even if continued for a long while, *may* not be satisfactory, because the fresh ice that I have described must have been formed at least twelve months, perhaps eighteen months, before.

The Transposition of Boulders from below to above the Ice.

When boulders, small stones, sand, gravel, &c. are found lying on sea-ice, it is very generally supposed that they must have rolled down a steep place or fallen from a cliff, or been deposited by a flow of water from a river or other source. There is, however, another way in which boulders &c. get upon floe-ice, which I have not seen mentioned in any book on this subject.

During the spring of 1847, at Repulse Bay on the Arctic shores of America, I was surprised to observe, after the thaw commenced, that large boulders (some of them 3 or 4 feet in diameter) began to appear on the surface of the ice; and after a while, about the month of July, they were wholly exposed, whilst the ice below them was strong, firm, and something like 4 feet thick.

There were no cliffs or steep banks near from which these boulders could have come; and the only way in which I could account for their appearance, was that which by subsequent observation I found to be correct.

On the shores of Repulse Bay the rise and fall of the tide are 6 or 8 feet, sometimes more. When the ice is forming in early winter, it rests, when the tide is out, on any boulders &c. that may be at or near low-water mark. At first, whilst the

ice is weak, the boulders break through it; but when the ice becomes (say 2 or 3 feet) thick, it freezes firmly to the boulder, and when the tide rises, is strong enough to lift the boulder with it. Thus, once fastened to the ice, the stone continues to rise and fall with the rise and fall of each tide, until, as the winter advances, it becomes completely enclosed in the ice, which by measurement I found to attain a thickness of more than 8 feet.

Small stones, gravel, sand, and shells may be fixed in the ice in the same way.

In the spring, by the double effect of thaw and evaporation, the upper surface of the ice, to the extent of 3 feet or more, is removed, and thus the boulders, which in autumn were lying at the bottom of the sea, are now on the ice, while it is still strong and thick enough to travel with its load, before favourable winds and currents to a great distance.

The finding small stones and gravel on ice out to sea does not always prove that such ice has been near the shore at some time or other.

I have noticed that wherever the Walrus in any numbers have been for some time lying either on ice or rocks, a not inconsiderable quantity of gravel has been deposited, apparently a portion of the excreta of that animal, having probably been taken up from the bottom of the sea and swallowed along with their food.

Mammoth-remains. The position in which their Skeletons are found, &c.

In Lyell's 'Principles of Geology,' vol. i. p. 185, we read:—"In the flat country near the mouth of the Yenesei river, Siberia, between latitudes 70° and 75° north, many skeletons of mammoths, retaining the hair and skin, have been found. The heads of most of these are said to have been turned to the south."

As far as I can find, the distinguished geologist gives no reason why the heads of the mammoths were turned to the south; nor does he say all that I think might be said of the reasons why, and the means by which the skins have been preserved for such a long period of time.

Having lived some years on the banks of two of the great rivers of America, near to where they enter Hudson's Bay, and also on the M'Kenzie, which flows into the Arctic Sea, I have had opportunities of observing what takes place on these streams, all of which have large alluvial deposits, forming flats and shallows at their mouths.

What I know to be of common occurrence in these rivers may, if we reason by analogy, have taken place in ancient times

on the great rivers of Siberia, making due allowance for the much higher northern latitude to which these streams run before reaching the sea, and for the difference in size of the fauna that used to frequent their banks.

When animals, more especially those having horns, tusks, or otherwise heavily weighted heads, are drifting down a river, the position of the bodies may lie in any direction as regards the course of the stream, as long as they are in water deep enough to float them; but the moment they get into a shallow place, the head, which sinks deepest (or, as sailors say, "draws most water"), takes the ground, whilst the body, still remaining afloat, swings to the current, just as a boat or ship does when brought to anchor in a tideway.

It is probable that the mammoths, having been drowned by breaking through the ice or in swimming across the river in spring when the banks were lined with high precipitous drifts of snow, which prevented them from getting out of the water, or killed in some other way, floated down stream, perhaps for hundreds of miles, until they reached the shallows at the mouth, where the heads, loaded with a great weight of bone and tusks, would get aground in 3 or 4 feet of water, whilst the bodies still afloat would swing round with the current as already described.

The Yenesei flows from south to north, so the heads, being pointed up stream, would be to the south*.

Supposing, then, these bodies anchored as above in 3 or 4 feet water; as soon as the winter set in, they would be frozen up in this position. The ice in so high a latitude as 70° or 75° north would acquire a thickness of 5 or 6 feet at least, so that it would freeze to the bottom on the shallows where the mammoths were anchored. In the spring, on the breaking up of the ice, this ice being solidly frozen to the muddy bottom, would not rise to the surface, but remain fixed, with its contained animal remains, and the flooded stream would rush over both, leaving a covering of mud as the water subsided.

Part of this fixed ice, but not the whole, might be thawed away during summer; and (possibly, but not necessarily) next winter a fresh layer of ice with a fresh supply of animal remains might be formed over the former stratum; and so the peculiar position and perfect state of preservation of this im-

* Not many years ago, when buffalo were very abundant on the Saskatchewan, hundreds of them were sometimes drowned in one season whilst swimming across the river; and many reindeer, moose, and other animals are annually destroyed in this way in other large American rivers.

Sir Charles Lyell mentions a number of yaks being seen frozen up in one of the Siberian rivers, which, on the breaking up of the ice in spring, would be liberated and float down the stream.

mense collection of extinct animals may be accounted for without having recourse to the somewhat improbable theory that a very great and sudden change had taken place in the climate of that region.

I have seen at the mouth of Hayes River in America animals frozen up as above described; but as the latitude of this place is only 57° north, the fixed ice usually wholly disappears before the next winter sets in, and liberates the animals shut up in it; but when the rivers reach the sea, as some of those of Siberia do, 1000 or 1200 miles further to the north, it may be fairly assumed that a large part of this fixed ice, protected as it would be by a layer of mud, might continue unthawed.

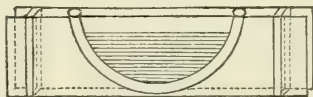
IX. *Glass Cell with Parallel Sides.*

By F. CLOWES, *Esq., B.Sc., F.C.S.**

THE following method has proved very convenient for making a glass cell, which may be readily fitted up from ordinary laboratory apparatus, and may also be rapidly taken to pieces for the purpose of being cleansed.

A piece of india-rubber tubing with stout walls, or, better, a length of solid rubber, is placed in the form of a letter U between two plates of glass, the ends of these plates being then firmly held together by slipping over them stout india-rubber rings.

Fig. 1.



A glass cell is thus obtained, the parallel faces of which are formed by the glass plates, whilst its thickness, depth, and length can be suitably varied by the stoutness and length of the rubber tube and the shape which this tube is made to assume.

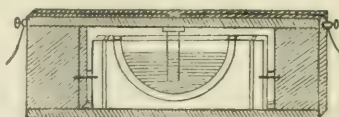
With a glass cell of the size of an ordinary magic-lantern slide (fig. 1), the difference in specific gravity between hot and cold water† may be well shown upon the screen by a magic lantern, the liquid admitted by a pipette being preferably tinged by dissolving in it a crystal of potassium permanganate; and the convective currents occurring in the mass of a liquid may be thrown upon the screen by passing a galvanic current through a fine platinum wire stretched between two thick copper wires beneath the surface of the liquid in the cell: these currents are rendered much more evident by allowing the platinum wire to be immersed in a stratum of potassium-permanganate solution which has been cautiously introduced beneath the water by means of a pipette dipping to the bottom of the cell.

* Read before the Physical Society, May 23, 1874. Communicated by the Society.

† See Tyndall's 'Heat, a Mode of Motion,' pp. 173 and 174.

A smaller cell made to fit into the wooden frame of a lantern-slide (fig. 2), which has attached to it platinum wires connected by copper wires and binding-screws with a galvanic battery, serves to project electrolytic decompositions upon the screen.

Fig. 2.



Perhaps the most beautiful appearance is that presented by the crystallization of the metal from a solution of lead-acetate which is undergoing electrolysis*.

In order that the cell may be water-tight, it is necessary that the india-rubber rings should exert a somewhat powerful compression; but even under favourable circumstances slight leakage is liable to occur in about half an hour after the cell has been filled; this, however, would allow ample time for the display of any of the phenomena above alluded to. Rings cut from large-sized india-rubber tubing have been found well adapted for the construction of small cells.

X. Notices respecting New Books.

Text-Books of Science.—Principles of Mechanics. By T. M. GOOD-
EVE, M.A., Lecturer on Applied Mechanics at the Royal School of
Mines. London: Longmans, Green, and Co. 1874 (small 8vo,
pp. 313).

THIS book contains an exposition of the principles of mechanics, such as is commonly given in elementary treatises on that science. The exposition is illustrated in two ways—*first* by means of examples of the ordinary type, *secondly* by reference to actual mechanical contrivances mainly of a modern character. There are about a hundred and eighty illustrations of the former kind; and of these about one in every four is taken from the Science Examination papers drawn up for the annual examinations of the Department of Science and Art. The second class of illustrations constitutes the chief peculiarity of the book, and unquestionably its most valuable part. The mere names of some of these illustrations will be enough to show this—*e. g.* the carrying of corn on bands, the feeding of running trains with water, the disintegrating flour-mill, the ventilation of coal-mines, the lifting of coals, the stone-crushing machine, Weston's friction coupling, the break-drum, the crown valve, the blowing-engine, the hydraulic accumulator, the hydraulic crane, &c. These form an assemblage of contrivances which have never before, to our knowledge at least, been described in any elementary book; they render the work before us worthy of the study of all who are interested in mechanical science; and we do not

* Mr. W. Crookes, F.R.S., suggests the electrolysis of solution of thallium sulphate as furnishing a still more beautiful example of crystallization.

doubt that these illustrations alone will cause the book to have, as it undoubtedly deserves to have, an extensive circulation.

It will be evident from the large number of contrivances mentioned in the above list, that the description of each must be brief, and that the attention of the reader is mainly directed to the dynamical principles involved in their use. It could scarcely fail to happen, under these circumstances, that in some cases points in the contrivances are not quite so fully described as the reader might wish. In others the contrivance is regarded from a point of view which does not bring quite the whole subject under notice; and this is sometimes a little misleading. For instance, the contrivance for feeding a running train with water is considered simply as an illustration of inertia; and this probably accounts for the statement that the water which runs up the tube "is at rest except so far as the movement in a vertical direction is concerned" (p. 49). As one end of the tube is vertically over the other end, it is plain that the water before it leaves the tube must have acquired the forward velocity of the train as well as the vertical velocity with which it ascends the tube; and in fact the illustration of the inclined plane pushed beneath the water (p. 49), if properly worked out, shows this very point: *e. g.* conceive a particle (P) at rest acted on by no forces, and an inclined plane (with an angle α) moving forward with a velocity V to come into contact with it; an instantaneous action takes place between the plane and the point along the perpendicular to the plane; and after the action, P will move with a uniform velocity along a line in space coinciding with the position of the perpendicular at the instant of the action. If we further suppose that there is no force of restitution, P , while moving in space along the above-mentioned line, will continue to touch the plane and appear to run up it. Supposing the mass of the plane large in comparison with that of P , the horizontal and vertical components of P 's velocity will be $V \sin^2 \alpha$ and $V \sin \alpha \cos \alpha$. It is evident from the former expression that, if the plane were steep, the forward horizontal velocity of P would be nearly equal to V , and would be quite equal to it if the plane were vertical. The velocities would be increased if there were restitution, and the point would be thrown forward from the plane, of course along the aforesaid perpendicular. This is true supposing P to be not acted on by any other force than the momentary action of the plane; if we suppose P to be under the action of gravity, the above velocities are its horizontal and vertical initial velocities, and the subsequent motion can be easily determined on the usual suppositions. Now the contrivance for feeding running trains with water differs from the case we have been considering in this—that instead of a mere inclined plane, a tube with a gradually increasing slope is employed; the effect of this is threefold: in the *first place*, the increasing slope makes the action gradual instead of instantaneous, thereby diminishing the tendency of the instrument to dash the water out of the trough; in the *next place*, if the water, when once in the tube, have any tendency to fly forward owing to restitution or any

other cause, the tendency has no effect so far as the present question is concerned; and, finally, as the tube for a large part of its length is nearly or quite vertical, the horizontal velocity of the ascending stream cannot fail to acquire the forward velocity of the train.

The Statement of General Principles and the proofs of particular theorems contained in the text are (it is almost needless to say so) correct as far as we have noticed; and the student who works at the book conscientiously will doubtless not fail to make it out, though the style does not generally show in any marked degree the power of clear exposition. There is one point which ought not to be left unnoticed, as the author lays considerable stress upon it: he states that he has endeavoured "above all to show that the relation of the theory of heat to mechanics should be approached by the student in his earliest inquiries with the same careful thought with which he will surely regard it when his knowledge and his powers have become extended and strengthened." And accordingly the book contains articles in which are explained what is meant by the mechanical equivalent of heat, by the kinetic theory of gases, and one or two other matters. What parts of a subject an author puts into his book is a matter depending so much on his own judgment as to be rarely the proper subject of criticism; but we may perhaps be allowed to record a difference of opinion. It seems to us, then, that the subject of energy of motion presents difficulties to the beginner so great that it is best to give him a fair chance of becoming familiar with it before introducing him to the far more difficult subject of Potential Energy, and accordingly that it is better not to deal with the latter subject in a purely elementary treatise on mechanics.

Eclipses Past and Future, with General Hints for Observing the Heavens.
By the Rev. S. J. JOHNSON. Parker & Co.: Oxford and London.
1874.

Mr. Johnson, in the work before us, has added considerably to our prospective knowledge of eclipses, transits, and allied phenomena, and has also given us some interesting information relative to ancient eclipses, mentioning that the first of which we have a clear record happened at Nineveh in the year 763 B.C. Noticing in the order of their sequence the most celebrated eclipses of antiquity, and bringing up the catalogue of observed eclipses to the present date, the author gives us two interesting chapters (V. and VI.):—the first on the prospects of the amateur, showing the paucity of large eclipses in England during the next thirty years; and the second, "Curiosities in Lunar Eclipses," as bright and black total eclipses, and those in which both luminaries were above the horizon at the time of the moon being eclipsed, an obvious effect of refraction. The first part of the work, in which we have notices of eclipses from the celebrated one of Ho and Hi 2127 B.C. October 13, to A.D. 2381 July 21, contains a large amount of information on an interesting branch of astronomy.

From eclipses of the Sun and Moon, the author passes in the second part of his work to describe prospectively the most interesting planetary phenomena, the periods at which they may be most advantageously looked for, with the peculiar features they are likely to present. Allusions are made to the Aurora, Zodiacal Light, Meteors, &c.; and we notice a remarkable suggestion embodied in a communication to the 'Spectator' by the Rev. E. L. Garbett, that the cities of Sodom and Gomorrah were destroyed by a group of the meteors following Tempel's telescopic comet of 1866. Mr. Garbett gives six reasons for his suggestion as follows:—

1. From the deduced period of node passage of the comet a visit must have occurred in the autumn between B.C. 1898 and B.C. 1897, which is generally assumed as the date of the catastrophe.

2. The earth's passage of node was on July 31.

3. A vertical fall of meteors as rain was only possible at sunrise, the hour of the destruction of the cities.

4. The latitude of the vertical fall agrees with that of the cities.

5. Sodium, the chief element in the deposits formed in the locality, is the chief element in these meteors as observed by Secchi.

6. Magnesium, which also occurs in the locality, is the only other ingredient in the meteors conspicuous to Secchi by means of the spectroscope.

"Suppose," says the writer, "any event not due to this comet to be recorded. The chances against the account presenting these six agreements with its elements and no disagreements, are three millions to one that the history of Sodom is true, and this the physical cause."

The work closes with a list of 152 double stars and nebulae, arranged much in the same way as the portion on the Starry Heavens of Webb's 'Celestial Objects for Common Telescopes,' the angles of position of the double stars, as seen near the meridian, being indicated by dots, an addition which we have no doubt will be duly appreciated by those readers who are just commencing their observational career.

XI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xlvii. p. 457.]

December 11, 1873.—Joseph Dalton Hooker, C.B., President, in the Chair.

THE following communication was read:—

"On the Action of Heat on Gravitating Masses." By William Crookes, F.R.S. &c.

The experiments recorded in this paper have arisen from observations made when using the vacuum-balance, described by the author in his paper "On the Atomic Weight of Thallium"* , for

* Phil. Trans. 1873, vol. clxiii. p. 277.

weighing substances which were of a higher temperature than the surrounding air and the weights. There appeared to be a diminution of the force of gravitation; and experiments were instituted to render the action more sensible, and to eliminate sources of error.

In an historical *résumé* of the state of our knowledge on the subject of attraction or repulsion by heat, it is shown that in 1792 the Rev. A. Bennet recorded the fact that a light substance delicately suspended in air was attracted by warm bodies: this he ascribed to air-currents. When light was focused, by means of a lens, on one end of a delicately suspended arm, either in air or in an exhausted receiver, no motion could be perceived distinguishable from the effects of heat.

Laplace spoke of the repulsive force of heat. Libri attributed the movement of a drop of liquid along a wire heated at one end, to the repulsive force of heat; but Baden Powell did not succeed in obtaining evidence of repulsion by heat from this experiment.

Fresnel described an experiment by which concentrated solar light and heat caused repulsion between one delicately suspended and one fixed disk. The experiment was tried in air of different densities; but contradictory results were obtained under apparently similar circumstances at different times, and the experiments were not proceeded with.

Saigey described experiments which appeared to prove that a marked attraction existed between bodies of different temperatures.

Forbes, in a discussion and repetition of Trevelyan's experiment, came to the conclusion that there was a repulsive action exercised in the transmission of heat from one body into another which had a less power of conducting it.

Baden Powell, repeating Fresnel's experiment, explained the results otherwise than as due to repulsion by heat. By observing the *descent* of the tints of Newton's Rings between glass plates when heat was applied, Baden Powell showed that the interval between the plates increased, and attributed this to a repulsive action of heat.

Faye introduced the hypothesis of a repulsive force of heat to account for certain astronomical phenomena. He described an experiment to show that heat produced repulsion in the luminous arc given by an induction-coil in rarefied air.

The author describes numerous forms of apparatus successively more and more delicate, which enabled him to detect and then to render very sensible an action exerted by heat on gravitating bodies, which is not due to air-currents or to any other known form of force.

The following experiment with a balance made of a straw beam with pith-ball masses at the ends enclosed in a glass tube and connected with a Sprengel pump, may be quoted from the paper:—

"The whole being fitted up as here shown, and the apparatus being full of air to begin with, I passed a spirit-flame across the lower part of the tube at *b*, observing the movement by a low-power micrometer; the pith ball (*a, b*) descended slightly, and then immediately rose to considerably above its original position. It

seemed as if the true action of the heat was one of attraction, instantly overcome by ascending currents of air.

"31. In order to apply the heat in a more regular manner, a thermometer was inserted in a glass tube, having at its extremity a glass bulb about $1\frac{1}{2}$ inch in diameter; it was filled with water and then sealed up. . . The water was kept heated to 70° C., the temperature of the laboratory being about 15° C.

"32. The barometer being at 767 millims. and the gauge at zero, the hot bulb was placed beneath the pith ball at *b*. The ball rose rapidly; as soon as equilibrium was restored, I placed the hot-water bulb above the pith ball at *a*, when it rose again, more slowly, however, than when the heat was applied beneath it.

"33. The pump was set to work; and when the gauge was 147 millims. below the barometer, the experiment was tried again; the same result, only more feeble, was obtained. The exhaustion was continued, stopping the pump from time to time, to observe the effect of heat, when it was seen that the effect of the hot body regularly diminished as the rarefaction increased, until when the gauge was about 12 millims. below the barometer the action of the hot body was scarcely noticeable. At 10 millims. below it was still less; whilst when there was only a difference of 7 millims. between the barometer and the gauge, neither the hot-water bulb, the hot rod, nor the spirit-flame caused the ball to move in an appreciable degree. The inference was almost irresistible that the rising of the pith was only due to currents of air, and that at this near approach to a vacuum the residual air was too highly rarefied to have power in its rising to overcome the inertia of the straw beam and the pith balls. A more delicate instrument would doubtless show traces of movement at a still nearer approach to a vacuum; but it seemed evident that when the last trace of air had been removed from the tube surrounding the balance—when the balance was suspended in empty space only—the pith ball would remain motionless, wherever the hot body were applied to it.

"34. I continued exhausting. On next applying heat, the result showed that I was far from having discovered the law governing these phenomena; the pith ball rose steadily, and without that hesitation which had been observed at lower rarefactions. With the gauge 3 millims. below the barometer, the ascension of the pith when a hot body was placed beneath it was equal to what it had been in air of ordinary density; whilst with the gauge and barometer level its upward movements were not only sharper than they had been in air, but they took place under the influence of far less heat; the finger, for example, instantly sending the ball up to its fullest extent."

A piece of ice produced exactly the opposite effect to a hot body.

Numerous experiments are next given to prove that the action is not due to electricity.

The presence of air having so marked an influence on the action of heat, an apparatus was fitted up in which the source of heat (a platinum spiral rendered incandescent by electricity) was inside the

vacuum-tube instead of outside it as before; and the pith balls of the former apparatus were replaced by brass balls. By careful management and turning the tube round, the author could place the equipoised brass pole either over, under, or at the side of the source of heat. With this apparatus it was intended to ascertain more about the behaviour of the balance during the progress of the exhaustion, both below and above the point of no action, and also to ascertain the pressure corresponding with this critical point.

After describing many experiments with the ball in various positions with respect to the incandescent spiral, and at different pressures, the general result is expressed by the statement that the tendency in each case was to bring the centre of gravity of the brass ball as near as possible to the source of heat, when air of ordinary density, or even highly rarefied air, surrounded the balance. The author continues :—

“44. The pump was then worked until the gauge had risen to within 5 millims. of the barometric height. On arranging the ball above the spiral (and making contact with the battery), the attraction was still strong, drawing the ball downwards a distance of 2 millims. The pump continuing to work, the gauge rose until it was within 1 millim. of the barometer. The attraction of the hot spiral for the ball was still evident, drawing it down when placed below it, and up when placed above it. The movement, however, was much less decided than before; and in spite of previous experience (33, 34) the inference was very strong that the attraction would gradually diminish until the vacuum was absolute, and that then, and not till then, the neutral point would be reached. Within one millimetre of a vacuum there appeared to be no room for a change of sign.

“45. The gauge rose until there was only half a millimetre between it and the barometer. The metallic hammering heard when the rarefaction is close upon a vacuum commenced, and the falling mercury only occasionally took down a bubble of air. On turning on the battery current, there was the faintest possible movement of the brass ball (towards the spiral) in the direction of attraction.

“46. The working of the pump was continued. On next making contact with the battery, no movement could be detected. The red-hot spiral neither attracted nor repelled. I had arrived at the critical point. On looking at the gauge I saw it was level with the barometer.

“47. The pump was now kept at full work for an hour. The gauge did not rise perceptibly; but the metallic hammering sound increased in sharpness, and I could see that a bubble or two of air had been carried down. On igniting the spiral, I saw that the critical point had been passed. The sign had changed, and the action was faint but unmistakable *repulsion*. The pump was still kept going, and an observation was taken from time to time during several hours. The repulsion continued to increase. The tubes of the

pump were now washed out with oil of vitriol*, and the working was continued for an hour.

“48. The action of the incandescent spiral was now found to be energetically *repellent*, whether it was placed above or below the brass ball. The fingers exerted a repellent action, as did also a warm glass rod, a spirit-flame, and a piece of hot copper.”

In order to decide once for all whether these actions really were due to air-currents, a form of apparatus was fitted up which, whilst it would settle the question indisputably, would at the same time be likely to afford information of much interest.

By chemical means the author obtained in an apparatus a vacuum so nearly perfect that it would not carry a current from a Ruhmkorff's coil when connected with platinum wires sealed into the tube. In such a vacuum the repulsion by heat was still found to be decided and energetic.

An experiment is next described, in which the rays of the sun, and then the different portions of the solar spectrum, are projected on to the delicately suspended pith-ball balance. *In vacuo* the repulsion is so strong as to cause danger to the apparatus, and resembles that which would be produced by the physical impact of a material body.

Experiments are next described in which various substances were used as the gravitating masses. Amongst these are ivory, brass, pith, platinum, gilt pith, silver, bismuth, selenium, copper, mica (horizontal and vertical), charcoal, &c.

The behaviour of a glass beam with glass ends in a chemical vacuum, and at lower exhaustion, is next accurately examined when heat is applied in different ways.

On suspending the light index by means of a cocoon fibre in a long glass tube furnished with a bulb at the end, and exhausting in various ways, the author finds that the attraction to a hot body in air, and the repulsion from a hot body *in vacuo* are rendered still more apparent.

Speaking of Cavendish's celebrated experiment, the author says that he has experimented for some months on an apparatus of this kind, and gives the following outline of one of the results he has obtained:—

“A heavy metallic mass, when brought near a delicately suspended light ball, attracts or repels it under the following circumstances:—

“I. *When the ball is in air of ordinary density.*

a. If the mass is *colder* than the ball, it *repels* the ball.

b. If the mass is *hotter* than the ball, it *attracts* the ball.

“II. *When the ball is in a vacuum.*

a. If the mass is *colder* than the ball, it *attracts* the ball.

b. If the mass is *hotter* than the ball, it *repels* the ball.”

The author continues:—“The density of the medium surround-

* This can be effected without interfering with the exhaustion.

ing the ball, the material of which the ball is made, and a very slight difference between the temperatures of the mass and the ball, exert so strong an influence over the attractive and repulsive force, and it has been so difficult for me to eliminate all interfering actions of temperature, electricity, &c., that I have not yet been able to get distinct evidence of an independent force (not being of the nature of heat) urging the ball and the mass together.

“Experiment has, however, showed me that, whilst the action is in one direction in dense air, and in the opposite direction in a vacuum, there is an intermediate pressure at which differences of temperature appear to exert little or no interfering action. By experimenting at this critical pressure, it would seem that such an action as was obtained by Cavendish, Reich, and Baily should be rendered evident.”

After discussing the explanations which may be given of these actions, and showing that they cannot be due to air-currents, the author refers to evidences of this repulsive action of heat, and attractive action of cold, in nature. In that portion of the sun's radiation which is called heat, we have the radial repulsive force, possessing successive propagation, required to explain the phenomena of comets and the shape and changes of the nebulae. To compare small things with great—to argue from pieces of straw up to heavenly bodies—it is not improbable that the attraction, now shown to exist between a cold and a warm body, will equally prevail when, for the temperature of melting ice is substituted the cold of space, for a pith ball a celestial sphere, and for an artificial vacuum a stellar void. In the radiant molecular energy of cosmical masses may at last be found that “agent acting constantly according to certain laws,” which Newton held to be the cause of gravity.

January 8, 1874,—Joseph Dalton Hooker, C.B., President, in the Chair.

The following communication was read :—

“On Electrotorsion.” By George Gore, F.R.S.

This communication contains an account of a new phenomenon (of rods and wires of iron becoming twisted while under the influence of electric currents), and a full description of the conditions under which it occurs, the necessary apparatus, and the methods of using it.

The phenomenon of torsion thus produced is not a microscopic one, but may be made to exceed in some cases a twist of a quarter of a circle, the end of a suitable index moving through a space of 80 centimetres (=31 inches). It is always attended by emission of sound.

The torsions are produced by the combined influence of helical and axial electric currents, one current passing through a long copper-wire coil surrounding the bar or wire, and the other, in an axial direction, through the iron itself. The cause of them is the combined influence of magnetism in the ordinary longitudinal direc-

tion induced in the bar by the coil-current, and transverse magnetism induced in it by the axial one.

The torsions are remarkably symmetrical, and are as definitely related in direction to electric currents as magnetism itself. The chief law of them is—*A current flowing from a north to a south pole produces left-handed torsion, and a reverse one right-handed torsion* (i. e. in the direction of an ordinary screw). Although each current alone will produce its own magnetic effect, sound, and internal molecular movement, neither alone will twist the bar, unless the bar has been previously magnetized by the other. Successive coil-currents alone in opposite directions will not produce torsion, neither will successive and opposite axial ones.

The torsions are influenced by previous mechanical twist in the iron, by mechanical tension, and by terrestrial magnetic induction. The direction of them depends both upon that of the axial and of the coil-currents, but appears to be determined most by the former. A few cases occur in which the currents, instead of developing torsion, produce detorsion; but only two instances, out of many hundreds, have been met with in which torsion was produced in a direction opposite to that required by the law.

Single torsions vary in magnitude from 0·5 millim. to nearly 30 millims. of movement of the end of an index 47 centimetres long; the smaller ones occur when the two currents are transmitted alternately, and the large ones when they are passed simultaneously; the former generally leave the bar in a twisted state, the latter do not. Those produced by axial currents succeeding coil ones are nearly always much larger than those yielded by coil-currents succeeding axial ones, because the residual magnetism left by the coil-current is the strongest. The order of succession of the currents affects the torsions in all cases, altering their magnitudes, and in some few instances even their directions. In steel all the torsional effects are modified by the mechanical and magnetic properties of that substance.

Each current leaves a residuary magnetic effect in the bar, amounting in iron to about one tenth of its original influence. The residuary magnetism of coil-currents is affected and sometimes reversed by axial ones; and that of axial currents is also removed by coil ones, and by a red heat. The condition left by an axial current is smaller in degree and less stable, in a vertical iron wire or one in the terrestrial magnetic meridian, than that left by a coil one, partly because of the influence of terrestrial magnetism; but in a position at right angles to that the effect is different.

The torsion produced by a coil-current may be used as a test, and partly as a measure, of the residuary effect of an axial one; and that produced by an axial current may be employed to detect, and to some extent measure, ordinary magnetism in the bar. As an opposite coil-current at once reverses the ordinary longitudinal magnetism of a bar of iron, so also an opposite axial one at once reverses its transverse magnetism.

Many instances have been met with in which the transverse and

longitudinal magnetic states produced by the two currents coexisted in the same substance. The torsional influence of the excited helix is distributed equally throughout its length; so also is that of the current in the bar. All the torsions are closely related to the well-known electric sounds, and to particular positions and internal movements of the particles of the iron.

Signs of electrotorsion were obtained with a bar of nickel, but not with wires of platinum, silver, copper, lead, tin, cadmium, zinc, magnesium, aluminium, brass, or German-silver, nor with a thick rod of zinc, or a cord of gutta serena.

GEOLOGICAL SOCIETY.

[Continued from vol. xlvii. p. 462.]

June 25, 1873.—Joseph Prestwich, Esq., F.R.S., Vice-President, in the Chair.

The following communications were read:—

1. "On six Lake-basins in Argyllshire." By His Grace the Duke of Argyll, K.T., F.R.S., President.

The author referred to the part ascribed to glacial action in the formation of lake-basins, and described the basins of six lakes in Argyllshire, the characters presented by which seemed to him inconsistent with their having been excavated by ice. Among these lakes were Loch Fyne, Loch Awe, Loch Leckan, and the Dhu Loch. The upper part of Loch Fyne was said to be cut off from the rest by a bar of islands, with only one or two deeper passages. The country about Loch Fyne was described as consisting of Upper and Lower Silurian mica-slates, which have been violently contorted, their normal strike being indicated by the direction of the valleys. Loch Fyne occupies a niche in the slope of the rocks, having an escarpment on one side and the shelving strata on the other. The existence of a fault along the line of the loch was probable, but could not easily be ascertained. Its greatest depth in this part was said to be 84 fathoms. Its banks show marks of glaciation, whereon the surface is well adapted for their preservation; the strongest marks are on those rock-faces which look up the loch. Between Loch Fyne and Loch Awe the mica-slates are interstratified with granite, which the author believed to have been forced up between the plains of stratification by the pressure caused by the falling in of the mica-slates, as fragments of the latter rock are imbedded in the granite. The author described the different structure of the two banks of Loch Awe, the upper part of which seemed to him to lie in a synclinal trough; and its waters were only prevented by a low col from finding their way to the Atlantic in this direction, instead of from the lower end. The formation of the basin of Loch Awe seemed to the author to be due solely to geological structure, as was also the case with another lake beyond the head of Loch Awe. The surrounding country was said to be full of smaller lake-basins, the formation of which might be due to the denudation of the softer mica-schists lying below the

granite ridges. But in some cases the basins were excavated in the latter; Loch Leckan was mentioned as an example. It is about a mile long, from 100 to 200 yards broad, and no less than 18 fathoms deep. At the top of its southern bank, which consists of granite, there is another lake (Loch-na-Craig), about 200 yards broad and 9 fathoms deep. The surrounding hills are low, and there appeared to be no source which could furnish ice to excavate a lake of such depth as Loch Leckan; and further, the author contended that if one of these two basins had been excavated by ice, the other could hardly have been preserved intact. Two other lakes, excavated on the summits of granite ridges, were mentioned; and the author could not conceive how either a glacier or an ice-cap could have produced such basins. The Dhu Loch, separated from Loch Fyne by a bank of gravel about a mile broad, is entirely in detrital matter, which the author thought might have been accumulated in its present form by the sea beating against the end of a glacier. From its position and level, the Dhu Loch rises and falls with the tide; and it would appear that it formerly extended some miles further up the valley, where the author had found clays containing a mixture of marine and fresh-water Diatomaceæ. In five of these cases the author thought it was impossible that the basins are due to glacial action.

2. "Description of the Skull of a dentigerous Bird (*Odontopteryx toliapicus*, Owen), from the London Clay of Sheppey." By Prof. Richard Owen, F.R.S., F.G.S.

The specimen described by the author consisted of the brain-case, with the basal portion of both jaws. The author described in detail the structure and relations of the various bones composing this skull, which is rendered especially remarkable by the denticulation of the alveolar margins of the jaws, to which its generic appellation refers. The denticulations, which are intrinsic parts of the bone bearing them, are of two sizes,—the smaller ones about half a line in length, the larger ones from two to three lines. The latter are separated by intervals of about half an inch, each of which is occupied by several of the smaller denticles. All the denticles are of a triangular or compressed conical form, the larger ones resembling laniaries. Sections of the denticles show under the microscope the unmistakable characters of avian bone. The length of the skull behind the fronto-nasal suture is 2 inches 5 lines; and from the proportions of the fragment of the upper mandible preserved, the author concluded that the total length of the perfect skull could not be less than between 5 and 6 inches. The author proceeded to compare the fossil, which he declared to present strictly avian characters, with those groups of birds in which the beak is longer than the true cranium, a character which occurs as a rule in the *Aves aquaticæ*. He stated that none of the Waders have the nostrils so remote from the orbits as in *Odontopteryx*; and this character, with the absence of the superorbital gland-pit, limits the comparison to the Totipalmates and Lamellirostrals. The former are excluded by their not having the orbit bounded by a hind wall as in *Odontopteryx*; and in this and other peculiarities the fossil seems to approach most nearly

to the Anatidæ, in the near allies of which, the Goosanders and Mergansers, the beak is furnished with strong pointed denticulations. In these, however, the tooth-like processes belong to the horny bill only; and the author stated that the production of the alveolar margin into bony teeth is peculiar, so far as he knows, to *Odontopteryx*. He concluded, from the consideration of all its characters, "that *Odontopteryx* was a warm-blooded, feathered biped, with wings; and further, that it was web-footed and a fish-eater, and that in the catching of its slippery prey it was assisted by this pterosauroid armature of its jaws." In conclusion, the author indicated the characters separating *Odontopteryx* from the Cretaceous fossil skull lately described by Prof. O. C. Marsh, and which he affirms to have small, similar teeth implanted in distinct sockets.

3. "Contribution to the Anatomy of *Hypsilophodon Foxii*, an Account of some recently acquired Remains of this Dinosaur." By J. W. Hulke, Esq., F.R.S., F.G.S.

After referring to Professors Owen and Huxley's descriptions of the Mantell-Bowerbank skeleton in the British Museum, and to the paper by the last-named gentleman on the skull of this Dinosaur read at a meeting of this Society in 1870, the author communicated details of its dentition, the form of its mandible, and that of the cones of the shoulder and fore limb, and of the haunch and hind limb, hitherto imperfectly or quite unknown. The resemblance to *Iguanodon* is greater than had been supposed; but the generic distinctness of *Hypsilophodon* holds good.

4. "On the Glacial Phenomena of the 'Long Island,' or Outer Hebrides." By James Geikie, Esq., F.R.S.E., F.G.S., of H.M. Geological Survey of Scotland.—First paper.

The author commenced by describing the physical features of Lewis, which he stated to be broken and mountainous in the south, whilst the north might be described as a great peat moss rising gradually to a height of about 400 feet, but with the rock breaking through here and there, and sometimes reaching a higher elevation. The north-east and north-west coasts are comparatively unbroken; but south of Aird Laimisheadar in the west and Stornoway in the east, many inlets run far into the country. The island contains a great number of lakes of various sizes, which are most abundant in the southern mountain tract and in the undulating ground at its base. The greater part of Lewis consists of gneiss, the only other rocks met with being granite and red sandstone, and conglomerate of Cambrian age. The stratification of the gneissic rocks is generally well-marked; the prevalent strike is N.E. and S.W., with S.E. dip, generally at a high angle. The author described in considerable detail the traces of glaciation observed in the lower northern part of Lewis, and inferred from his observations that the ice passed from sea to sea across the whole breadth of this district, and that it not only did not come from the mountainous tract to the south, but must have been of sufficient thickness to keep on its course towards the north-west undisturbed by the pressure of the glacier masses which must at the same time have filled the glens and valleys of that

mountain-region. After describing the characters presented by the bottom-till in the northern part of Lewis, the author proceeded to notice those of the lakes, some of which trend north-west and south-east, others north-east and south-west, whilst those of the mountain district follow no particular direction. The lake-basins of the first series he regarded as formed at the same time and by the same agency as the *roches moutonnées* and other marks of glacial action; they are true rock-basins or hollows between parallel banks formed wholly of till, or of till and rock. The N.E. and S.W. lakes coincide in direction precisely with the strike of the gneiss; and the author explained their origin by the deposition of till by the land-ice in passing over the escarpments of the gneiss facing the north-west. The lakes of the mountain district are regarded by the author as all produced by glacial erosion. The author considered that the ice which passed over the northern part of Lewis could only have come from the mainland. Referring to the glaciation of Raasay, he showed that the ice-sheet which effected it must have had in the Inner Sound a depth of at least 2700 feet; and taking this as approximately the thickness of the *mer de glace* which flowed into the Minch, which is only between 50 and 60 fathoms in depth, no part of this ice could have floated, and the mass must have pressed on over the sea-bottom just as if it had been a land surface. Ice coming from Sutherland must have prevented the flow of the Ross-shire ice through the Minch into the North Atlantic, and forced it over the low northern part of Lewis; and the height to which Lewis has been glaciated seems to show that the great ice-sheet continued its progress until it reached the edge of the 100-fathom plateau, 40 or 50 miles beyond the Outer Hebrides, and then gave off its icebergs in the deep waters of the Atlantic.

5. "Notes on the Glacial Phenomena of the Hebrides." By J. F. Campbell, Esq., F.G.S.

This communication consisted of notes extracted from the author's journal, giving his observations of indications of glacial action in various islands of the group of the Hebrides. Heynish in Tiree is 500 feet high, and has many large perched blocks on its top. These blocks are of gneiss; and the author thought they came from the north-west. The Barra islands are described as rocky, and resembling the hill-tops of a submerged land. All ice-marks found by the author seemed to him to come from the north and west. He thought that the final grinding was given by floating ice when the land was more submerged than at present. At Castle Bay, in Barra, the author observed well-preserved glacial striæ at the sea-level in a direction from N.N.W. The whole island is glaciated and strewn with perched blocks. Glacial indications were also observed in South Uist, Benbecula, and Skye; and the author stated that, on the whole, he was inclined to think that the last glacial period was marine, and that heavy ice came in from the ocean, the local conditions being like those of Labrador. The author regarded most of the lake-basins of the Hebrides as formed by ice-action, and considered that the ice by which those islands were glaciated came from Greenland.

6. "On Fossil Corals from the Eocene Formation of the West Indies." By Prof. P. Martin Duncan, M.B., F.R.S., V.P.G.S.

The author had considered his labours amongst the fossil corals of the West-Indian Islands finished; but lately a very fine collection has been sent to him from the University of Upsala, and Mr. P. T. Cleve of Stockholm. The specimens were collected from limestone and coral conglomerates, which are covered by and rest upon volcanic debris and ejectamenta in the Island of St. Bartholomew. The species represented there are numerous, and may be divided into:—Group 1, species not hitherto known; 2, species with a Cretaceous facies; 3, species characteristic of the horizons of the Upper Eocene and Oligocene deposits of Europe; 4, species found also in the Nummulitic deposits of Europe and Sinde; 5, species belonging to the recent coral fauna; 6, species belonging to genera which belong to the Jurassic fauna, and to the Caribbean.

The determination of the forms of the associated Mollusca and Echinodermata permit the following deposits being placed on a general geological horizon—the limestone and conglomerate of St. Bartholomew, the dark shales beneath the Miocene of Jamaica, the beds of San Fernando, Trinidad. These were probably contemporaneous with the Java deposits, the Eocene of the Hala chain, the great reefs of the Castel Gomberto district, the reefs of Oberberg in Steiermark, and the Oligocene of Western Europe.

The author has already described reef corals from the Lower Cretaceous (Upper Greensand) of Jamaica; and the size of the specimens proves that the reef was exposed to the surf of an open sea. To these reefs succeeded on the same area others in the Eocene time, in the Miocene and Pliocene; and there are modern reefs in the neighbourhood.

The affinities and identities of the fossil forms with those of contemporaneous reefs in Asia and Europe, and the limitation of the species of the existing Caribbean coral fauna, point out the correctness of the views put forth by S. P. Woodward, Carriek Moore, and the author, concerning the upheaval of the Isthmus of Panama after the termination of the Miocene period.

7. "Note on the Lignite-deposit of Lal-Lal, Victoria, Australia." By R. Etheridge, Esq., Jun., F.G.S.

The author described this deposit, which is worked at the village of Lal-Lal, south of Mount Bunniyong. A boring towards the centre of the deposit showed about 73 feet of sand, clay, and gravel, 3 feet of fireclay, and 115 feet of lignite. The lignite is an earthy bituminous coal, composed of branches, roots, &c. of coniferous trees. In the mass there are a few thin seams of jet and clay-beds, accompanied by two kinds of resin. The lignite is very poor in carbon. It is almost entirely composed of remains of coniferous plants not now existing in Victoria; and the author considered that it is nearly of the same age as the Lignite deposit of Morrison's Diggings, which has been regarded as Miocene.

XII. *Intelligence and Miscellaneous Articles.*

ON THE FLOW OF SALINE SOLUTIONS THROUGH CAPILLARY TUBES.

BY THEODORE HÜBENER.

THE velocity of the flow of solutions in capillary tubes appears not to depend solely on their weight and capillary adhesion. Poiseuille has demonstrated that the velocity of flow of a mixture of water and alcohol decreases in proportion as the specific gravity increases by the addition of larger and larger quantities of water, to a minimum which corresponds exactly to the maximum of contraction of the mixture. Girard found that the velocity of flow of chloride of sodium is less than that of a solution of chloride of potassium of the same density.

M. Hübener thought that, beside the adhesion and the weight of the liquid, an important factor for the velocity of flow of a solution must be the intermolecular friction resulting from its greater or less cohesion; and to test this he has compared the velocities of a number of solutions of very different chemical compositions brought to the same density.

The liquid was introduced into a vertical rectilinear glass tube of 50 centims. length and 1.78 centim. diameter, having a capillary continuation of about 40 centims. length. The large tube presented two marks; and with a seconds-watch the time was accurately measured which was required for the level of the liquid to fall from one of these marks to the other.

Operating in this way upon solutions of chloride, bromide, and iodide of potassium, of chloride of sodium and of ammonium, with a density of 1.059 and at a fixed temperature, the author ascertained that *the velocity of flow of saline solutions is as much lower as the atomic weight of the salt dissolved is less*. For the different binary bodies above indicated, it is the metal which has the greatest influence upon the velocity of flow, much more than the metalloid. The variations presented by the velocity from one body to another are as much more marked as the tube is more capillary and as the concentration of the solution is greater.

On comparing two solutions, of chloride of sodium and potassium, at 1.1058 density, the author arrived at the remarkable result that the times of flow of these two salts are found to be very sensibly proportional to their equivalents. From this experiment, and from others analogous, extended also to the chlorides of the alkaline-earthly metals barium, strontium, magnesium, M. Hübener thinks it may be concluded generally with a high degree of probability, that *the velocities of flow of these bodies in solution in water, to a certain degree of concentration, are in the same ratio as their equivalents*.

The explanation of these facts is, according to M. Hübener, to be found in the circumstance that the molecules of substances which have a higher equivalent are larger, but, on the other hand, in less number, and consequently must give rise to less friction with the solvent in which they are held, thus communicating greater

mobility to the solution.—*Bibliothèque Universelle, Archives des Sciences Phys. et Nat.* No. 197, pp. 75, 76.

A NOTE ON MELDE'S EXPERIMENT. BY W. LOWERY.

In performing Melde's experiment upon the vibrations of strings, it is desirable to change the tension of the vibrating cord in a continuous manner. The ordinary method of attaching weights to the cord does not admit of this with precision; and with small weights the movement of the weight itself, on account of the rapid vibration of the string, prevents the formation of the ventral segments with regularity. I have adopted the following method:—A glass tube graduated into millimetres is weighted so as to float in a vertical position: this is attached to the silk cord which hangs from the prong of the tuning-fork, and is placed in a glass vessel filled with water. This latter vessel is provided with a siphon, by means of which the water can be drawn off at pleasure. It will be readily seen that, by drawing off the water from the larger vessel, the displacement produced by the graduated glass tube is diminished, and the tension of the string thereby is increased. By diminishing or increasing the amount of water in the larger vessel the tension can be diminished or increased to the desired extent.

In order to make quantitative experiments, the tube is in the first place connected with the arm of a delicate hydrostatic balance. The balance is adjusted when the level of the water in which the tube floats is at the zero of the millimetre scale. In order to avoid errors in reading, it is best to use a cathetometer. The weights which are necessary to keep the index of the balance at zero, when the level of the water in the outer vessel falls through the millimetre divisions on the graduated tube, are noted. The upward pressure of the water, and consequently the tension upon the suspending cord, are then given in grams.

In order to show the regularity of the method, the following results of one experiment are given. In the experiments, a glass tube which, immersed at 110 millims. on the scale, weighed two grams gave, when the level of the water in the outer vessel was lowered, the following:—

Immersed at 110 millims.		Weight = 2 grams.	
"	102 "	"	2.5 "
"	93.5 "	"	3 "
"	85 "	"	3.5 "
"	76.5 "	"	4 "
"	67.5 "	"	4.5 "
"	60 "	"	5 "
"	43 "	"	5.5 "

In these experiments a fall of 8.1 millims. corresponded to a difference of .5 of a gram. It is evident by increasing the size of the outer vessel that a large amount of water would measure a slight displacement. When the cord was set in vibration, the following results were obtained:—

Point of immersion.	Weights in grams.	Vibrations.
110	2	6
84	3.5	5
76	4	4
30	6.7	3

The ratio of the numbers in the second and third columns will be found to follow Melde's law.

For qualitative or quantitative experiments upon beats or Lissajous curves this method of loading the prong of a tuning-fork can advantageously replace the bit of wax or the sliding weight, since we have at our command a quick and precise method of adjustment. —Silliman's *American Journal*, May 1874.

ON CONSTANT ELECTRIC CURRENTS. BY M. HEINE, OF HALLE.

Kirchhoff* has developed a simple expression for the electric potential, with a constant current, in every point P of a circular homogeneous plate into which the current enters at given points A_1, A_2, \dots . If each letter E represents a constant depending on the strength of the current entering at the point A_i , and if B_i is the conjugate point to A_i , the electric potential of the circle in the point P becomes

$$V = \sum E_i \log (PA_i \cdot PB_i), \dots\dots\dots (\alpha)$$

when the summation is extended to all the points of inflow. Two points A, B of the circle are called conjugate which lie on the same right line MAB starting from the centre M, if the radius forms the mean proportional between MA and MB.

I have found the expression of the potential also for plates of other shapes, and will here give it for the ellipse and the rectangle.

Let the excentricity of the ellipse be 1; let the fourth power of the difference of the semiaxes (of which the greater represents the axis of the real, the smaller that of the imaginary) be put $=q$. Let each point z of the ellipse be described by the elliptic function

$$\operatorname{sn} \left(\frac{2K}{\pi} \operatorname{arcsin} z \right),$$

therefore the entire ellipse upon a circle with the radius $\frac{1}{\sqrt{k}}$ (as M.

Schwarz has shown). If now a, p are the images of the inflow-points A and an arbitrary point P of the ellipse, and if b denotes the point in the circle of radius $\frac{1}{\sqrt{k}}$ conjugate to a , the electric po-

tential of the ellipse in the point P will be

$$V = \sum E_i \log (pa_i \cdot pb_i). \dots\dots\dots (\beta)$$

If, lastly, we have a rectangle OXNY, whose base OX has the length π and is the axis of the real, and its height OY equals $-\log q$ and is the axis of the imaginary, we construct for each point of inflow A the three reflected images B, C, D which arise when A is assumed to be luminous, OX and OY reflecting $(x+yi, x-yi, -x-yi, -x+yi)$. If now each point z be represented by

* Pogg. Ann. vol. lxiv. p. 497, vol. lxxvii. p. 344.

$sn^2 \frac{Kz}{\pi}$, and thereby A, B, C, D, and the arbitrary point P of the rectangle fall upon points a, b, c, d, p , the electric potential of the rectangle in the point P is

$$V = \sum E_i \log (pa_i . pb_i . pc_i . pd_i) (\gamma)$$

Quinke* bases his experiments, on the potential in very large square plates when the points of inflow are in the diagonal, upon a formula of approximation which in our notation would be

$$V = \sum E_i \log (PA_i . PB_i . PC_i . PD_i).$$

It is now apparent, if this be compared with the exact formula (γ), that it results from the latter, if snz may be supposed proportional to z , therefore with very large rectangular plates—or, better, under the supposition that P and the A's lie near an angular point of the rectangle. The approximation-formula therefore holds also when the rectangle is not a square and when the inflow-points do not lie on the diagonal.

The derivation of these expressions I intend to communicate, in a connected form, to Borchardt's *Journal für Mathematik*. For this reason I omit here the exhibition in a purely analytical form, without the aid of geometry, of the relations expressed by (β) and (γ).—*Monatsbericht der königlich preussischen Akademie der Wissenschaften zu Berlin*, March 5, 1874.

ON THE NATURE OF THE ACTION OF LIGHT UPON SILVER BROMIDE. BY M. CAREY LEA, PHILADELPHIA.

When silver bromide is exposed for a moment to light, it undergoes no visible change, but has acquired the property of passing to an intense black when treated with pyrogallie acid and an alkali.

As to the nature of this black substance, there has existed considerable diversity of opinion. In a paper published on the subject about a year since by Captain Abney, F.C.S., he expressed the opinion that it was an oxide of silver.

Some years since, while investigating the action of light upon silver iodide, I succeeded in proving that the black substance which is produced when silver iodide is exposed to light in presence of silver nitrate contains iodine, and is therefore either a subiodide or an oxy-iodide. The quantity obtained was too small to enable me to ascertain which. When this black substance was treated with nitric acid, normal yellow silver iodide was left behind, and silver was found on solution.

I have recently applied the same treatment to the bromine compound with similar results. I find that when silver bromide is treated with pyrogallie acid and alkali after exposure to light, the black substance which remains contains bromine, and is resolved by nitric acid into normal silver bromide (left behind as a pale yellow film) and silver, which passes into solution. It is therefore either a subbromide or an oxy-bromide, not an oxide, probably the former.

The existence of these compounds is evidently an argument for doubling the atomic weight of silver, as has recently been proposed on other grounds.—Silliman's *American Journal*, May 1874.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

AUGUST 1874.

XIII. *On Attraction and Repulsion accompanying Radiation.*

By WILLIAM CROOKES, F.R.S. &c.*

[With a Plate.]

BEFORE describing the apparatus and experiments which illustrate the attraction and repulsion accompanying radiation, it will perhaps be best to draw attention to the modification of the Sprengel pump which has so materially assisted me in this investigation.

Fig. 1 (Plate I.) shows the pump as now in use. Working so much with this instrument, I have endeavoured to avoid the inconveniences attending the usual mode of raising mercury from the lower to the upper reservoir. The mercury is contained in a closed glass reservoir A, perforated with a fine hole at the top. This reservoir is attached to a block capable of free movement in a vertical line and running in grooves, and connected with the lower reservoir by a flexible tube *g*. This tubing is specially made to stand a considerable pressure of mercury. It consists of a double thickness of india-rubber tubing enclosing a canvas tube in the centre, the whole being vulcanized together.

When the whole of the mercury has run through the pump, the reservoir and slide can be lowered by liberating a detent, T, and letting it descend to the block L. H is a glass reservoir which receives the mercury after flowing through the pump. When the reservoir A is emptied and has been lowered to the block L, the mercury from H is admitted into A by opening the tap I. At *f* is another tap, of platinum, to regulate the flow of mercury through the pump. *c*, *c*, *c'* are mercury joints, it

* A Lecture delivered before the Physical Society, June 20, 1874. Communicated by the Society.

being inconvenient to have the apparatus in one piece of tubing, and not always possible to seal the different portions together by fusion. *ee* is a barometer dipping into the same vessel as the gauge-barometer *P*, the two thus forming a differential system by which the rarity of the atmosphere in the apparatus undergoing exhaustion can be easily estimated. *dd* is a millimetre-scale with pointed end, attached to the gauge and capable of being raised or lowered so as to make the point just touch the surface of the mercury. *b* is a reservoir of strong sulphuric acid, exposing as much surface as possible, but allowing the air to pass across it without resistance. The mercury joint *c'* may either be closed with a piece of glass rod ground in, or it may have either of the two pieces of apparatus *i* and *k* fitted to it. *k* is a mercurial siphon gauge, which is useful for measuring very high rarefactions in experiments where difference of pressure equal to a tenth of a millimetre of mercury is important. *i* is for still higher rarefactions; it is simply a small tube having platinum wires sealed in, and intended to be attached to an induction-coil. At exhaustions beyond the capabilities of the mercurial gauge I can still get valuable indications of the nearness to a perfect vacuum by the resistance of this tube. I have frequently carried exhaustion to such a point that an induction-spark will not strike across the small distance ($\frac{1}{4}$ inch) separating the wires of the vacuum-tube. *h* is the mercury-tap usually employed for letting air into the apparatus, and also for moistening the interior of the pump with oil of vitriol. *l* is a spiral of glass for attaching the various pieces of apparatus requiring exhaustion. As blown or fused joints are indispensable, this form of connecting piece is adopted to ensure the necessary flexibility. *m* is a trap to catch any air which might leak in through the platinum tap *f*, or the various joints in the lower part of the tubing *g*.

The reservoir *A* being filled with mercury, the tap *I* is turned off and the reservoir is raised to the top of the slide where it is supported by the detent *T*. On opening the tap *f* the mercury rises in the tube *f h*, and, falling through the chamber *N*, carries with it the air contained in the tube *R*, and in the apparatus attached to the tube *l*, as in the ordinary Sprengel pump. At *N* the tubing is enlarged in order that the mercury may not be forced up the tube *R*, as otherwise frequently happens if the tubes or the mercury gets soiled.

J, J are iron brackets supporting the apparatus. *S* is a large inverted glass receiver, to collect the small portions of mercury which are unavoidably and constantly being spilled; it should contain a little weak alkaline solution.

The part of the tubing *g, f, h, N* forms a barometric siphon

arrangement, which effectually prevents air getting into the pump from the reservoir A when the mercury has completely run out. In this case no harm whatever is done to the operation: the vacuum is not injured; and the exhaustion proceeds immediately on retransferring the mercury from the reservoir H to the reservoir A, and raising A again into its place. The apparatus, as thus arranged, is readily manageable with certainty of obtaining a barometric vacuum.

The mercury fall-tube of a pump in constant use frequently wants cleaning. I find the most effectual means of doing this is to put oil of vitriol into the funnel *h*, and then, by slightly loosening the glass stopper, allow a little of the strong acid to be carried down the tube with the mercury. With care this can be effected without interfering with the progress of exhaustion. The residual acid adhering to the walls of the chamber N does good rather than harm. When sufficient sulphuric acid has run into the fall-tube, the funnel-stopper can be perfectly closed by pressing it in with a slight twist and then filling up with mercury.

Many physicists have worked on the subject of attraction and repulsion by heat. In 1792 the Rev. A. Bennet recorded the fact that a light substance delicately suspended in air was attracted by warm bodies; this he ascribed to air-currents. When, by means of a lens, light was focused on one end of a delicately suspended arm, either in air or in an exhausted receiver, no motion could be perceived distinguishable from the effects of heat. After Mr. Bennet the subject has been more or less noticed by Laplace, Libri, Fresnel, Saigey, Forbes, Baden Powell, and Faye; but the results have been unsatisfactory and contradictory.

My first experiments were performed with apparatus made on the principle of the balance. An exceedingly fine and light arm is delicately suspended in a glass tube by a double-pointed needle; and at the ends are affixed balls of various materials. Amongst the substances thus experimented on I may mention pith, glass, charcoal, wood, ivory, cork, selenium, platinum, silver, aluminium, magnesium, and various other metals. The beam is usually either of glass or straw.

The apparatus, consisting of a straw beam and pith-ball ends, being fitted up as here shown attached to the pump, and the whole being full of air to begin with, I pass a spirit-lamp across the upper part of the tube just over one of the pith-balls. The ball rises. The same effect is produced when a bulb of hot water, or even the warm finger is placed over the pith-ball.

On working the pump and repeating the experiment, the attraction to the hot body gets less and less, until it becomes *nil*; and after a certain barometric pressure is passed, the attraction

gives place to repulsion, which gets stronger and stronger as the vacuum approaches perfection.

In order to illustrate more strikingly the influence exerted by a trace of residual air, an apparatus (fig. 2) is here shown in which the source of heat (a platinum spiral, *a*, rendered incandescent by electricity) is inside the glass tube instead of outside it as before. A mass of magnesium, *b*, turned conical, is suspended in a glass tube, *cde*, by a fine platinum wire of such a length as to vibrate seconds. The upper end of the platinum wire is sealed into the glass at *e*, and passes through to the outside for the purpose of electrical experiments. The platinum spiral is arranged so that when the pendulum hangs free the magnesium mass is about $\frac{1}{4}$ inch from it. In air the red-hot spiral produces decided attraction on the magnesium; and by properly timing the contacts with the battery, a considerable swing can be accumulated. On perfectly exhausting the apparatus, however, the incandescent spiral is found to energetically repel, and a very few contacts and breaks properly timed are sufficient to get up the full swing the pendulum is capable of.

A simpler form of the apparatus for exhibiting the phenomena of attraction in air and repulsion in a vacuum consists of a long glass tube *ab* (fig. 3) with a globe *c* at one end. A light index of glass with pith-balls at the ends *d, e* is suspended in this globe by means of a cocoon fibre. When the apparatus is full of air at ordinary pressure, a ray of heat or light falling on one of the pith-balls gives a movement indicating attraction.

When the apparatus is exhausted until the barometric gauge shows a depression of 12 millims. below the barometer, neither attraction nor repulsion results when radiant light or heat falls on the pith. When the vacuum is as good as the pump will produce, strong repulsion is shown when radiation is allowed to fall on one end of the index. The heat of the hand, or even of the body several feet off, is quite sufficient. The action is in proportion to the surface acted on rather than to the mass.

The barometric position of the neutral point dividing attraction from repulsion varies with the density of the mass on which radiation falls, on the ratio of its mass to its surface, and in a less degree on the intensity of radiation. In the case of pith it is seen to lie at about 12 millims. below a barometric vacuum, whilst with a heavy metal it is within a tenth of a millim. of a vacuum. Experiments to try to determine the law governing the position of the neutral point are now in progress.

Ice, or a cold substance, produces the opposite effects to heat. Thus a bar of pith suspended in a vacuum is energetically repelled by the warm hand, whilst it is as strongly attracted by a piece of ice. Cold being simply negative heat, it is not easy at

first sight to understand how it can produce attraction. The law of exchanges, however, explains this perfectly. The pith index and the whole of the surrounding bodies are incessantly exchanging heat-rays; and under ordinary circumstances the income and expenditure of heat are in equilibrium. A piece of ice brought near one end of the index cuts off the influx of heat to it from that side, and therefore allows an excess of heat to fall upon it from the opposite side. Attraction by a cold body is thus seen to be only repulsion by the radiation from the opposite side of the room.

Instruments of the kind just described are perhaps the best for exhibiting large and striking movements of attraction or repulsion. Two glass globes 4 inches in diameter, fitted up with bars of pith $3\frac{1}{2} \times \frac{1}{2}$ inch, are now before you. One is full of air at ordinary pressure, whilst the other is completely exhausted. A touch with a finger on a part of the globe near one extremity of the pith will drive the bar round over 90° , in the vacuum. In air the attraction is not quite so strong.

If I place a lighted candle an inch or two from the vacuous globe, the pith bar commences to oscillate. The swing gradually increases in amplitude until one or two complete revolutions are made. The torsion of the suspending fibre here interferes, and the vibrations proceed in the opposite direction. The movement continues as long as the candle burns. This continued movement ceases if the source of radiation is removed some distance off; the pith index then sets equatorially. The cause of the continued vibration when the radiant body is at a particular distance from the pith is easy to understand on the supposition that the movement is due to the direct impact of waves on the suspended body.

For more accurate experiments I prefer making the apparatus differently. Fig. 4 represents the best form. *ab* is a glass tube, to which is fused at right angles another, narrower tube, *cd*; the vertical tube is slightly contracted at *e* so as to prevent the solid stopper *d*, which just fits the bore of the tube, from falling down. The lower end of the stopper *de* is drawn out to a point; and to this is cemented a fine glass thread about 0.001 inch diameter, or less, according to the torsion required.

At the lower end of the glass thread an aluminium stirrup and a concave glass mirror are cemented, the stirrup being so arranged that it will hold a beam *fg* having masses of any desired material at the extremities. At *c* in the horizontal tube is a plate-glass window cemented on to the tube. At *b* is also a piece of plate glass cemented on. Exhaustion is effected through a branch tube *h* projecting from the side of the upright tube. This is sealed by fusion to the spiral tube of the pump,

The stopper *d e*, and the glass plates *c* and *b*, are well fastened with a cement of resin 8 parts and bee's-wax 3 parts*.

The advantage of a glass-thread suspension is that the beam always comes back to its original position. Before you is an instrument of this description, perfectly exhausted and fitted up with pith plates at each extremity. A ray of light from the electric lamp is thrown on to the mirror *c*, and thence reflected on to the opposite wall. The approach of a finger to either extremity of the beam causes the luminous index to travel several feet, showing repulsion. A piece of ice brought near causes the spot of light to travel as much in the opposite direction.

Here is another form of the apparatus (fig. 5). The letters and description are the same as in fig. 4, the apparatus, however, being double. The pieces *f*, *g* on the end of one beam consist of platinum-foil exposing a square centimetre of surface, whilst the extremities *f'*, *g'* on the other beam consist of pith plates of the same size. It has already been explained that the neutral point of rarefaction for platinum is much higher than for pith; consequently at a pressure intermediate between these two neutral points, radiation ought to cause the platinum to be attracted and the pith to be repelled. This is seen to be the case. A wide beam of radiant heat thrown in the centre of the tube on to the plates *g*, *f'* causes *g* to be attracted and *f'* to be repelled, as shown by the light reflected from the mirrors *c*, *c'*. The atmospheric pressure in the apparatus is equal to about 40 millims. of mercury.

The position of the neutral point not only depends on the density of the body acted on by radiation, as in the above case, but also on the relation of surface to mass. Thus a square centimetre of thin platinum-foil on the extremity of the beam requires a lower exhaustion for neutrality than a thicker piece exposing the same surface. Also a flat disk of platinum has a lower neutral point than the same weight of platinum in the form of a sphere.

Intensity of radiation likewise affects the neutral point. With

* This is the best cement I have used for standing a vacuum: for a few hours it is perfect. But at the highest exhaustions it seems to leak in the course of a day or two. India-rubber joints are of no use in these experiments, as, when the vacuum is near upon perfect, they allow oxygenized air to pass through as readily as the pump will remove it. Whenever possible the glass tubes should be united by fusion; and where this is impracticable mercury joints should be used. The best way to make these is to have a well-made perforated conical stopper, cut from plain india-rubber, fitting into the wide funnel-tube of the joint and carrying the narrow tube. Before fitting the tubes in the india rubber this is heated in a spirit-lamp until its surface is decomposed and very sticky; it is then fitted into its place; mercury is poured over, and oil of vitriol on the top of that. When well made, this joint seems perfect.

pith extremities a point of rarefaction can be obtained at which the warm fingers repel and incandescent platinum attracts.

During the course of this lecture I have spoken frequently of repulsion by *heat*, and have used a spirit-lamp as a source of heat to illustrate the facts described. I now wish to show that these results are not confined to the heating rays of the spectrum, but that any ray, from the ultra-red to the ultra-violet, will produce repulsion in a vacuum.

In my own laboratory I have used sunlight, and have experimented with a very pure spectrum, taking precautions to avoid any overlapping or diffusion of one part of the spectrum with another. Here I can only use the electric light, and, in order to get results visible at a distance, the spectrum cannot be very long.

The spectrum is formed with one disulphide-of-carbon prism, and is projected on to the screen by a lens. Immediately behind the screen is an exhausted bulb, having a movable index with pith terminals suspended with a cocoon fibre (fig. 3). This is delicate enough to swing over 90° with a touch of the finger, and it will even move under the influence of a ray of moonlight. I first of all arrange the spectrum so that the extreme red would fall on one pith disk were it not for the screen. On removing the screen the index immediately retreats, making nearly half a revolution.

I now replace the screen, and arrange the spectrum so that the invisible ultra-violet rays are in a position to fall on the pith disk. On removing the screen the index at once behaves as it did under the influence of the red rays, and is driven away twenty or thirty degrees. The action is not so powerful as when the other end of the spectrum is used; but this may partly, if not wholly, be accounted for by the much greater concentration of energy at the red end of the spectrum, and expansion at the violet end, when using glass or disulphide-of-carbon prisms.

I now, without disturbing the position of the spectrum, interpose in the path of the rays a cell containing a solution of iodine in disulphide of carbon, which is opaque to the luminous and ultra-violet rays, but transparent to the invisible heat-rays. Not a trace of repulsion is produced. The iodine solution is now removed and the ultra-violet rays again fall on the pith, producing strong repulsion. A thick screen of clear alum cut from one of Mr. Spence's gigantic crystals is now interposed; but no effect whatever is produced by it, the ultra-violet rays acting with unabated energy. As alum cuts off all the dark heat-rays, this experiment and the one before it prove the sufficient purity of my spectrum.

The spectrum is again turned until the dark ultra-red heating

rays fall on the pith. The movement of repulsion is energetic. The iodine solution, interposed, cuts off apparently none of the action. The alum plate cuts off a considerable amount, but by no means all. On uniting the alum and the iodine solution the whole of the spectrum is obliterated, and no action is produced, whatever be the ray which would, were it not for this double sifting, fall on the pith.

Throughout the course of these investigations, which have occupied much of my spare time for some years, I have endeavoured to keep in my mind the possible explanations which may be given of the actions observed; and I have always tried, by selecting some circumstances and excluding others, to put each hypothesis to the test of experiment.

The most obvious explanation is, that the movements are due to the currents formed in the residual gas which theoretically must be present to some extent even in those vacua which are most nearly absolute.

Another explanation is, that the movements are due to electricity developed on the moving body or on the glass apparatus by the incident radiation.

A third explanation has been put forward by Professor Osborne Reynolds, in a paper which was read before the Royal Society on June 18th last. He considers the results to be due to evaporation and condensation.

I will discuss these explanations in order.

First, the air-current theory. However strong may be the reasons in favour of this explanation, they are, I think, answered irrefragably by the phenomena themselves. It is most difficult to believe that the residual air in a Sprengel vacuum, when the gauge and barometer are level, can exert, when gently warmed by the finger, an upward force capable of instantly overcoming the inertia of a mass of matter weighing 20 or 30 grains. It must be remembered that the upward current supposed to do this is simply due to the diminished weight of a portion of the gas caused by its increase in volume by the heat applied.

An air-current produced by heat may possibly cause the beam of a balance to rise, may drive a suspended index sideways, and by a liberal assumption of eddies and reflections, may perhaps be imagined to cause these movements to take place sometimes in the opposite directions; but as rarefaction proceeds these actions must certainly get less, and they will cease to be appreciable some time before a vacuum is attained: a point of no action or neutrality will be reached. But this neutral point should certainly be nearer to a vacuum when a light body like pith, exposing much surface, is under experiment,

than when the mass acted on is heavy like brass; whereas in practice the contrary obtains. Pith ceases to move under the influence of radiation at a rarefaction of about 7 to 12 millims., whilst brass only ceases to be affected when the gauge and barometer are appreciably level.

But even could the phenomena up to the neutral point be explained by air-currents, these are manifestly powerless to act after this critical point is passed. If a current of air within 7 millims. of a vacuum cannot move a piece of pith, certainly the residual air in a Sprengel vacuum should not have more power; and *à fortiori* the residual gas in a perfect chemical vacuum cannot possibly move a mass of platinum.

It is, however, abundantly demonstrated that, in all cases after this critical point is reached, the repulsion by radiation is most apparent; it increases in energy as the vacuum approaches perfection, and attains its maximum when there is no air whatever present, or at all events not sufficient to permit the passage of an induction-spark.

I will now refer to the electrical explanation. Very early in my investigation, phenomena were noticed which caused me to think that electricity played a chief part in causing the movements. When a hot glass rod is held motionless against the side of an exhausted tube containing a pith index, repulsion takes place in a perfectly regular manner; but if the glass rod has been passed once or twice through the fingers, or is rubbed a few times sideways against the exhausted bulb, the index immediately moves about in a very irregular manner, sometimes being repelled from, and at others attracted to, the side of the glass, where it adheres until the electrical excitement subsides. Friction with the finger produces the same results; and a small spirit-flame causes similar, but much fainter, electrical effects. I soon ascertained, however, that, although electricity is capable of producing many movements similar to those caused by radiation, they are never so alike as to be mistaken. Electricity frequently interferes with, disturbs, or neutralizes the true action of radiation; but it acts in such a manner as to show that it is not the primary cause of the movement. At the highest rarefactions, and when special precautions have been taken to avoid the presence of aqueous vapour, slight friction with the finger against the bulb, or a touch with the flame of a spirit-lamp, excites so much electrical disturbance in the pith and other indexes that accurate observations become impossible with them for several hours. I have tried many means of neutralizing the electrical disturbance; but they are only partially successful, and at the highest rarefactions interference through electrification is very troublesome.

I may draw attention to the following experiments, which are devised with the object of showing that the attractions and repulsions are not due to electricity.

In describing the pendulum apparatus (fig. 2) which I set in motion at the early part of this lecture, I explained that the mass of magnesium forming the weight was in metallic contact with the platinum wire which supported it, and that the upper end of this platinum wire was fused into the glass tube and passed through to the outside. With this apparatus I have tried a great number of experiments. I have connected the projecting end of the platinum wire with "earth," with either pole of an induction-coil the other being insulated more or less, with either pole of a voltaic battery, with a delicate electroscope; I have charged it with an electrophorus, and have submitted it to the most varied electrical conditions; and still, on allowing radiation to fall upon the suspended mass, I invariably obtain attraction when air is present, and repulsion in a vacuum. The heat has been applied both from the outside, so as to pass through the glass, and also inside by means of the ignited platinum wire; and the results have shown no difference in kind, but only in degree, under electrical excitement. I have obtained interference with the usual phenomena, but never of such a character as would lead me to imagine that the normal results were due to electricity.

It occurred to me that the repulsion might be due to a development of electricity on the inner surface of the glass bulb or tube under the influence of the radiation as it passed from the glass into the vacuum. This appears to be disproved by the fact that the results are exactly the same whether the radiation passes through the glass, or whether it is developed inside the apparatus as in the above instance.

I have produced exactly the same phenomena whether the exhausted apparatus has been standing insulated in the air, or whether it was completely immersed in water connected electrically with "earth," or surrounded with wet blotting-paper.

Here are two experiments which bear on this subject. A straw beam furnished with brass balls at each end is suspended on a double-pointed needle, and the brass balls and needle are placed in metallic connexion by means of fine platinum wire. The needle does not rest on the sides of the glass tube, but in steel cups, to which is soldered a platinum wire passing through the glass tube and connected with "earth." The tube is then exhausted, and the usual experiments are tried with hot and cold bodies, both with and without a wet blotting-paper cover. In all cases the moving beam behaves normally, being repelled by heat and attracted by cold.

An apparatus is prepared similar to that shown in fig. 4.

The inside of the tube *ab* is lined with a cylinder of copper gauze, having holes cut in the centre for the passage of the supporting thread *dc* and the index ray of light falling on the mirror *c*, and holes at each end to admit of the plates *f* and *g* being experimented with. A wire attached to the copper gauze passes through a hole in the plate *b*, so as to give me electrical access to the copper gauze lining. Under the most diverse electrical conditions, whether insulated or connected with "earth," this apparatus behaves normally when exhausted.

A further reason why electricity is not the cause of the movements I have described is, that they are not only produced by heat, but also by ice and cold bodies. Moreover I shall presently show that any ray of the spectrum, besides those red and ultra-red rays which produce dilatation of mercury in a thermometer, excite an electric current between antimony and bismuth couples, and cause a sensation of warmth when falling on the skin, will produce the effect of repulsion in a vacuum. It is therefore to my mind abundantly proved that electricity, such as we at present know this force, is not a chief agent in these attractions and repulsions, however much it may sometimes interfere with and complicate the phenomena.

I will now discuss Professor Osborne Reynolds's theory, that the effects are the results of evaporation and condensation. In my exhausted tubes he assumes the presence of aqueous vapour, and then argues as follows:—"When the radiated heat from the lamp falls on the pith, its temperature will rise, and any moisture on it will begin to evaporate and to drive the pith from the lamp. The evaporation will be greatest on that ball which is nearest to the lamp; therefore this ball will be driven away until the force on the other becomes equal, after which the balls will come to rest, unless momentum carries them further. On the other hand, when a piece of ice is brought near, the temperature of the pith will be reduced, and it will condense the vapour and be drawn towards the ice."

Professor Reynolds has tried an experiment with pith-balls attached to a light stem of glass and suspended by a silk thread in a glass flask. The exhaustion was obtained by boiling water in the flask and then corking it up and allowing it to cool. The gauge showed an exhaustion of from $\frac{1}{2}$ to $\frac{3}{4}$ of an inch. The pith-balls behaved exactly as I have already shown they do at that degree of exhaustion, heat repelling and ice attracting. He found that the neutral point varied according to whether air was present with the aqueous vapour, or whether the vapour was pure water-gas. Professor Reynolds states:—"From these last two facts it appears as though a certain amount of moisture on the balls was necessary to render them sensitive to the heat. . . . These ex-

periments appear to show that evaporation from a surface is attended with a force tending to drive the surface back, and condensation with a force tending to draw the surface forward."

It does not appear that Professor Reynolds has tried more than a few experiments; and he admits that they were in reality undertaken to verify the explanation above quoted. I have worked experimentally on this subject for some years; and the last experiment recorded in my notebook is numbered 584. From the abundant data at my disposal, I can find many facts which will, I think, convince you that this hypothesis has been arrived at on insufficient evidence.

In the first place, I will show that the presence of moisture or of a condensable vapour is not necessary. Besides pith, which from its texture and lightness might be supposed to absorb and condense considerable quantities of vapour, I have used glass, mica, and various metals; and with a proper amount of exhaustion they all act in the same manner. The fact that the neutral point for platinum is close upon a vacuum, whilst that for pith is so much lower, tends to show that the repulsion is not due to any recoil caused by condensable vapour leaving the surface under the influence of heat. Were it so, it would certainly require more vapour to be present when platinum had to be driven backwards than when pith had to be moved; but the contrary obtains in all cases. The rule seems to be, the greater the density the higher the neutral point.

I have worked with all kinds of vacua; that is to say, I have started with the apparatus filled with various vapours and gases (air, carbonic acid, water, iodine, hydrogen, &c.); and at the proper rarefaction I find no difference in the results which can be traced to the residual vapour. A hydrogen vacuum seems neither more nor less favourable to the phenomena than does a water vacuum, or an iodine vacuum.

If moisture be present to begin with, I find it necessary to allow the vapour to be absorbed by the sulphuric acid of the pump, and to continue the exhaustion, with repeated heating of the apparatus, until the aqueous vapour is removed. Then and then only do I get the best results.

When pith is employed as the index, it is necessary to have it thoroughly dried over sulphuric acid before using it, and during the exhaustion to keep it constantly heated to a little below its charring-point, in order to get the greatest sensitiveness.

Professor Reynolds says, "In order that these results might be obtained, it was necessary that the vapour should be free from air." On the contrary, I find the results take place with the greatest sharpness and rapidity if the residual gas consists of nothing but air or hydrogen.

Professor Reynolds further says, "Mr. Crookes only obtained his results when his vacuum was nearly as perfect as the Sprengel pump would make it. Up to this point he had nothing but the inverse effects, viz. attraction with heat and repulsion with cold." In the abstract of my paper published in the Proceedings of the Royal Society, I describe an experiment with a pith-ball apparatus in which the neutral point is 7 millims. (about $\frac{1}{4}$ inch) below the vacuum, repulsion by heat taking place at higher exhaustions. At the Royal Society *Soirée*, April 22, 1874, I showed, and fully described in print, the apparatus now before you, consisting of a pith bar suspended by a cocoon fibre in a glass bulb, from which the air is exhausted until the barometric gauge shows a depression of 12 millims. below the barometer. Neither attraction nor repulsion results when radiant light or heat falls on the pith. Exhaustions of 7 and 12 millims. are certainly very inferior vacua for a Sprengel pump.

As a matter of fact, however, I have obtained repulsion by radiation at far higher pressures than these. The true effect of radiation appears to be one of repulsion at any pressure, overbalanced when a gas is present by some cause—possibly air-currents, but probably not. I have already explained that the barometric height of this neutral point dividing attraction from repulsion varies with the density of the substance on which radiation falls, on the relation which the mass bears to the surface, and on the intensity of radiation. By modifying the conditions it is not difficult to get repulsion by radiation when the apparatus is full of air at nearly the normal pressure of the atmosphere.

Professor Reynolds again says, "The reason why Mr. Crookes did not obtain the same results with a less perfect vacuum was because he had then too large a proportion of air, or non-condensing gas, mixed with the vapour." On this I may remark that the writer, before he explained how it was I could *not* get certain results, should have made sure that what he assumed to be the case was really so. I have not the least difficulty in showing repulsion by heat in imperfect vacua when mixed vapours and gases are present.

In my arguments against the air-current theory, I have shown that the best results are obtained when the vacuum is so nearly perfect that an induction-spark will not pass through it. This is an equally good argument against the presence of a condensable vapour as it is against that of air.

From the construction of my Sprengel pump I am satisfied that the vapour of mercury is absent from the apparatus.

The following experiments have been specially tried with the object of testing this theory. A tolerably thick and strong bulb

is blown at the end of a piece of combustion-tubing; and in it is supported a bar of aluminium at the end of a long platinum wire. The whole is attached to the Sprengel pump, and exhaustion is kept going on for about two days, until a spark will not pass through the vacuum. During this time the bulb and its contents are frequently raised to an incipient red heat. At the end of that time the tube is sealed off, and the bar of aluminium is found to behave exactly as it would in a less perfectly exhausted apparatus; viz. it is repelled by heat. A similar experiment, attended with similar results, has been tried with a glass index. It is impossible to conceive that in these experiments sufficient condensable gas was present to produce the effects Professor Reynolds ascribes to it. After the repeated heatings to redness at the highest attainable exhaustion (the gauge and the barometer being level for nearly the whole of the 48 hours), it is impossible that sufficient vapour or gas should condense on the movable index to be instantly driven off, by the warmth of the finger, with recoil enough to drive backwards a heavy piece of metal.

My own impression is that the repulsion accompanying radiation is directly due to the impact of the waves upon the surface of the moving mass, and not secondarily through the intervention of air-currents, electricity, or evaporation and condensation. Whether the ætherial waves actually strike the substance moved, or whether at that mysterious boundary-surface separating solid from gaseous matter there are intermediary layers of condensed gas which, taking up the blow, pass it on to the layer beneath, are problems the solution of which must be left to further research.

In giving what I conceive to be reasonable arguments against the three theories which have been supposed to explain these repulsions, I do not wish to insist upon any theory of my own to take their place. The one I advance is to my mind the most reasonable, and as such is useful as a working hypothesis, if the mind must have a theory to rest upon. *Any* theory will account for *some* facts; but only the true explanation will satisfy *all* the conditions of the problem, and this cannot be said of either of the theories I have already discussed.

My object at present is to ascertain facts, varying the conditions of each experiment so as to find out what are the necessary and what the accidental accompaniments of the phenomena. By working steadily in this manner, letting each group of experiments point out the direction for the next group, and following up as closely as possible, not only the main line of research, but also the little bylanes which often lead to the most valuable results, after a time the facts will group themselves together

and tell their own tale. The conditions under which the phenomena invariably occur will give the laws; and the theory will follow without much difficulty. To use the eloquent language of Sir Humphry Davy, "When I consider the variety of theories which may be formed on the slender foundation of one or two facts, I am convinced that it is the business of the true philosopher to avoid them all together. It is more laborious to accumulate facts than to reason concerning them; but one good experiment is of more value than the ingenuity of a brain like Newton's."

XIV. Fourier's Theorem.

By JAMES O'KINEALY, *Bengal Civil Service*.*

THE proof given of Fourier's theorem in all the text-books I know of, is a modified form of that first given by Poisson. What is at present proposed is to prove it by an analytical process for periodic functions, and to show that it is simply the solution of an exponential differential equation.

If $f(x) = f(x + \lambda)$, where λ is the wave-length, we have, putting it into the symbolical form,

$$f(x) = \epsilon^{\lambda D_x} f(x),$$

or

$$(\epsilon^{\lambda D_x} - 1)f(x) = 0.$$

It is a well-known theorem in differential equations, that if we get an equation of the form $F(D_x)f(x) = 0$, and can find the roots of $F(D_x) = 0$, the equation can be put in the form

$$(D_x - a)(D_x - a_1)(D_x - a_2) \dots f(x) = 0,$$

where a, a_1, a_2 , &c. are the roots of $F(D_x) = 0$, and that the solution will be

$$f(x) = A\epsilon^{ax} + A_1\epsilon^{a_1x} + A_2\epsilon^{a_2x} \dots,$$

A, A_1, A_2 , &c. being constants depending on the nature of $f(x)$.

In the present case $F(D_x)$ is $\epsilon^{\lambda D_x} - 1$. Assume $D_x = z$, and the equation to solve is

$$\epsilon^{\lambda z} - 1 = 0,$$

or

$$\epsilon^{\frac{\lambda}{2}z} - \epsilon^{-\frac{\lambda}{2}z} = 0,$$

or

$$\sin \frac{\lambda z}{2\sqrt{-1}} = 0,$$

or

$$\frac{\lambda z}{2\sqrt{-1}} = \pm \pi i$$

* Communicated by the Author.

(where i is cipher or a positive integer), or

$$z = D_x = \pm \frac{2\pi i \sqrt{-1}}{\lambda}.$$

The original differential equation becomes thus

$$D_x \cdot \left(D_x + \frac{2\pi i \sqrt{-1}}{\lambda} \right) \left(D_x - \frac{2\pi i \sqrt{-1}}{\lambda} \right) \sin f(x) = 0,$$

or

$$f(x) = A + A_1 \cos \frac{2\pi x}{\lambda} + A_2 \cos \frac{4\pi x}{\lambda} + \dots$$

$$+ B_1 \sin \frac{2\pi x}{\lambda} + B_2 \sin \frac{4\pi x}{\lambda} \dots$$

$$= A + \sum_{i=1}^{i=\alpha} A_i \cos \frac{2\pi i x}{\lambda} \\ + \sum_{i=1}^{i=\alpha} B_i \sin \frac{2\pi i x}{\lambda}.$$

This is Fourier's theorem; and, determining the constants in the usual way by integrating between 0 and λ , and by multiplying by $\cos \frac{2\pi i x}{\lambda} \cdot \sin \frac{2\pi i x}{\lambda}$ and then integrating, we get the usual form,

$$f(x) = \frac{1}{\lambda} \int_0^\lambda f(x) \cdot dn + \frac{2}{\lambda} \sum_{i=1}^{i=\alpha} \cos \frac{2\pi i x}{\lambda} \int_0^\lambda f(x) \cos \frac{2\pi i x}{\lambda} \cdot dn \\ + \frac{2}{\lambda} \sum_{i=1}^{i=\alpha} \sin \frac{2\pi i x}{\lambda} \int_0^\lambda f(x) \sin \frac{2\pi i x}{\lambda} \cdot dn.$$

In the same way we can obtain other forms of Fourier's theorem. If $f(x) = f(x+h)$, we have generally $f(x) = f(x+nh)$, where n is an integer; or $(e^{nh\lambda} - 1)f(x) = 0$, which gives the same solution as above if we put $n\lambda$ in place of λ .

Hence we find

$$f(x) = \frac{1}{n\lambda} \int_0^{n\lambda} f(x) \cdot dn + \frac{2}{n\lambda} \int_0^{h\lambda} f(x) \cdot dn \cdot \cos \frac{2\pi x}{n\lambda} \cdot X \cos \frac{2\pi i x}{n\lambda} \&c. \\ = \frac{1}{n\lambda} \int_0^{n\lambda} f(x) \cdot dn + \frac{2}{n\lambda} \sum_{i=1}^{i=\alpha} \cos \frac{2\pi i x}{n\lambda} \cdot \int_0^{n\lambda} f(x) \cdot \cos \frac{2\pi i x}{n\lambda} \cdot dn \\ + \frac{2}{n\lambda} \sum_{i=1}^{i=\alpha} \sin \frac{2\pi i x}{n\lambda} \cdot \int_0^{n\lambda} f(x) \cdot \sin \frac{2\pi i x}{n\lambda} \cdot dn.$$

The above method of solution may be applied to other some-

what similar forms. Suppose

$$f(x + \lambda) + f(x) = 0,$$

or

$$(\epsilon^{\lambda D_x} + 1)f(x) = 0.$$

The roots of the equation $\epsilon^{\lambda D_x} + 1 = 0$ are

$$D_x = \pm \frac{\pi i \sqrt{-1}}{\lambda},$$

where i is any odd integer. Hence

$$f(x) = \sum_{i=1}^{i=\alpha} A_i \cos \frac{\pi i x}{\lambda} + \sum_{i=1}^{i=\alpha} B_i \sin \frac{\pi i x}{\lambda},$$

where i is an odd integer.

Several other theorems of a similar nature will readily suggest themselves as capable of similar treatment.

11 Elysium Row, Calcutta.

XV. The Constant Currents in the Air and in the Sea: an Attempt to refer them to a common Cause. By BARON N. SCHILLING, *Captain in the Imperial Russian Navy.*

[Continued from p. 38.]

B. ROTATION OF THE EARTH ON ITS AXIS.

IN the daily motion of the earth on its axis, every point of the surface describes a circle. These parallel circles become smaller and smaller from the equator to the pole. Now, as all points of the surface describe their circles in one and the same time of nearly 24 hours, it is evident that as the poles are approached the velocity of motion of the points diminishes, and this in the ratio of the cosines of the latitudes. As already mentioned in discussing Hadley's theory of the trade-winds, a body approaching the equator, continually coming into circles of greater velocity, will, in consequence of the law of inertia, have the tendency to perform its revolution more slowly than these; and hence the direction of motion of that body will undergo a westerly deflection. Conversely, a body moving from the equator will be continually meeting with parallel circles of less and less velocity, and hence will take a direction swerving eastwards. Since the commencement of the 18th century the correctness of this law has been admitted, and it has been made use of to explain the direction of the trade-winds and many other phenomena. The Academician von Baer, for example, ascribes it to

this deviation arising from the earth's rotation that, in the northern hemisphere, all the rivers which run in a meridional direction undermine their right banks—through which these banks are the high, but the left the low ones. The whirling motion of cyclones, or Buys-Ballot's law, and the deviation of the meridional currents of the ocean, are all explained by the axial rotation of the earth.

It is indubitable that the direction of every independent motion on the surface of the earth receives, through its rotation, a certain tendency to deviation; yet it seems to us that the amount of this deviation is only too often greatly overrated. It is usually assumed that, in consequence of inertia, air and water particles can retain for hours the velocity of the parallels which they have long left behind. In reality, however, the friction and resistance of other particles will more speedily overcome the inertia and compel the particles in motion soon to take up the new rotation-velocity of the parallel circles which they enter. It must also be remembered that the velocity of adjacent parallels only very gradually changes; and therefore, with a slow motion of the particles, the least friction will be sufficient to overpower the difference existing between neighbouring parallels. The true proportion between the friction and the tendency to conserve the earlier rotation-velocity is very difficult to determine accurately; nevertheless it seems clear to us that the deviation thereby occasioned in the direction of motion in a short time must always be very inconsiderable. The defenders of Hadley's theory will admit this, although they generally believe that, by itself slight, the deviation can, by continual repetition of the action, accumulate and so become gradually considerable. They think, namely, that a current of air or sea, originated by difference of temperature, flowing along the meridian, would, when a little deflected by the earth's rotation, continue to flow in this new direction, and so on. In other words, it is generally believed that the angle which a current makes with the meridian must continually increase with the duration of the current; and some go so far as to see, not only in the south-west wind of the middle latitudes of our hemisphere, but even in the north-west wind of the same, an antipolar current diverted from its direction by the rotation of the earth. But this evidently false conclusion results from the false assumption that a current once deflected would continue to flow in the new direction. It is forgotten that the rotation of the earth cannot effect a deviation of the direction unless a motion is present. If the motive force ceased to act, the current would soon cease also, being overpowered by friction and other resistance. It cannot, therefore, flow further in the deflected direction, but will always again

seek to proceed in the direction in which the force acts that produces the motion, therefore in that of the meridian. Now, according to the existing theory, two forces are constantly acting both upon trade-winds and the meridional currents of the ocean:—of which the one, the impelling force, springs from the difference of temperature of the equatorial and polar regions, and hence operates only in the meridian-direction; while the other, the rotating force of the earth, acts in consequence of the inertia of the particles, and always in the direction of the parallel circle, therefore at right angles to the motive force. If, then, both forces remained constantly unaltered, the direction of the current would also remain invariably the same; that is, the angle which the current forms with the meridian would neither increase nor diminish. In our case the action of the force in the meridional direction must be assumed to be constantly uniform; while the lagging behind, or the advance, called forth by the transition into other latitudes is more considerable in the higher than in the lower latitudes, because the parallel circles diminish only very gradually in the latter, but rapidly in the former. From this it follows that the deviation from the meridional direction would necessarily be greatest in the high latitudes, and *vice versâ*. Every current, of air or sea, springing from difference of temperature would therefore, when flowing toward the equator, come constantly nearer to the direction of the meridian; while a current flowing away from the equator must be continually adding a little to the angle which it forms with the meridian. According to Hadley's theory, the trade-winds, flowing to the equator, should therefore be continually approximating nearer to the direction of the meridian; instead of which we see just the opposite—that at their polar limit they blow from the north-east or south-east, according to the hemisphere, and as they near the equator they come ever nearer to coincidence with the direction of the parallel circles.

The sea-currents of the southern hemisphere also demonstrate that the earth's rotation has but little, if any, influence on the direction of currents. The warm currents (those of Brazil and Mozambique) lean to the east coasts of the continents, and are directed to the south-west, instead of deviating eastwards; while the cold currents (those of Peru and South Guinea, and the general current of the entire Antarctic Ocean) agree in directing their course to the north-east, instead of flowing south-west as required by the theory of the deviation of direction arising from the rotation of the earth. We see in this circumstance a proof that the influence of the rotation is in the whole very little, although the direction taken by the currents of the northern hemisphere appears to correspond entirely with that theory:

namely, the Gulf-stream and the Kurosiwo flow north-east ; and the cold currents of the seas of Japan and Greenland go south-west, just as the earth's rotation demands. But since the currents of the southern hemisphere, notwithstanding the ample space open to them, are quite unaffected by the rotation of the earth, we cannot but see that the direction of the currents in the northern hemisphere must be referred to other causes. The bend which the Gulf-stream makes again to the north by Cape Hatteras, after a considerable inclination to the east, seems also to speak in favour of our view.

If the rotation of the earth deflects the direction of the sea-currents only inconsiderably, then its influence on the direction of the rivers can also only be very slight ; but even the slightest friction of the water flowing along the bank, if constantly repeated on one side during thousands of years, must at length perceptibly undermine the bank ; and hence Von Baer's view may in this respect be right, notwithstanding the extreme slightness of the deflection of the current.

On the rotation of cyclones we will speak subsequently.

Besides the action, above considered, upon already existing currents, many ascribe to the earth's rotation the power to be independently the motive cause of a current. Mühry, for instance, seeks the force which impels the equatorial stream in the centrifugal force of the earth. But this, as we know, acts always in the direction of the radius of the different parallel circles, and hence cannot possibly either accelerate or retard the rotation-velocity of the water and the air. Mühry evidently adheres to Kepler's explanation of the origination of the equatorial current, by the water staying behind the general motion of the earth. This, however, contradicts all the laws of mechanics, and is therefore quite inadmissible. The water and the air adhere to the globe by the pressure of their weight, and must, in the course of the thousands of years during which the earth has turned on its axis, have very long since attained the same velocity, through friction, since velocity once acquired is not again lost so long as there is no resistance. The permanence of the earth's rotation, however, sufficiently proves that in the universe no such resistance is present. The phenomena, too, of both air- and ocean-currents absolutely contradict the assumption that the water and air are subject to a slower rotation than the earth itself.

If this assumption were correct, the atmosphere, being less dense, must be far more exposed to the action of the retardation than the water, and over the entire surface of the earth we should constantly have strong east winds. Besides, although decreasing from the equator to the poles, the retardation would

yet be perceptible everywhere. But this is by no means the case. At the equator itself, or in its vicinity, no such current is to be observed either in the air or in the water; nay, in the equatorial zone we find that the sea has a slight current flowing east; so that here, not only is no retardation to be traced, but the water moves faster than the earth turns. In the zones of the Sargasso-seas, again, and in the calms of the tropics, in both hemispheres, neither in air nor sea can any diminution of the rotation-velocity be perceived. Further polewards, especially between the 40th and 50th parallels of latitude, the constant west winds and currents flowing east testify that water and air move eastwards more rapidly than the earth rotates. This current in the atmosphere in the middle latitudes is explained by the anti-trades, of which we have already spoken. It expresses itself in the sea just as it does in the atmosphere; but as the anti-trade explanation is absolutely inapplicable to the water, Mühry accounts for the sea-current by the aspirating force of the equatorial stream. Why, however, this force has no action at all upon the zones of the Sargasso-seas, but goes round them in a wide arc, remains unexplained. Thus, for example, in the South Atlantic the action of this aspirating force stretches along the coast as far as the Cape of Good Hope, and thence across the ocean to the American shore. If the aspirating force of the Atlantic equatorial current were actually so great that its influence could make itself perceptible not only to the Cape of Good Hope, but also from there to the American coast, then it could not fail to lay hold of the Mozambique current at the Cape and lead it into the Atlantic to supply the equatorial stream. Yet, as is well known, at the Cape of Good Hope the Mozambique current makes a strikingly sharp bend to the east, and returns on a wide circuit to compensate the equatorial stream which flows in the southern part of the Indian Ocean, after first washing the west coast of Australia. The insufficiency of the explanation of currents by aspiration presents itself so distinctly that it is scarcely necessary to dwell longer on the subject.

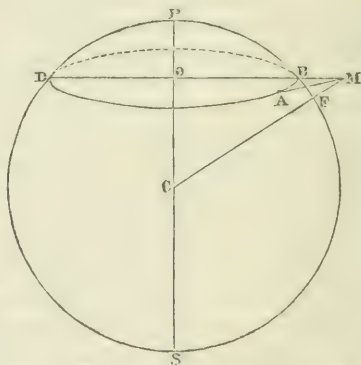
The earth's rotation cannot, then, generate any currents in air or sea; it can only effect a slight tendency of the freely moving air- and water-particles to shift their direction towards that of the equator. This tendency, however, is so feeble that from it no perceptible current can arise; hence we have not touched this point in the Russian edition of this work. We will nevertheless more closely consider here the action of the centrifugal force.

Every rotating motion, and therefore also that of the earth about its axis, generates a centrifugal force. The quantity of this force for every single point of the earth's surface is readily

determined by dividing the square of its rotational velocity by the radius of its parallel circle. It thence follows that the centrifugal force of the earth is greatest at the equator (0.11124 foot in a second), and from that to the poles it diminishes in the ratio of the cosines of the latitudes. By its action every thing on the surface of the earth would be thrown off, if the earth's gravitation were not greater than its centrifugal force. Now let us suppose the gravitation of the earth to cease to act during one second. Every particle not firmly adherent to the earth would instantly leave the surface and continue its motion in the direction of the tangent of the corresponding parallel circle with its previous rotational velocity; and the relative distance of the particle at the end of the second, from its point of separation, which the rotation has meanwhile carried forward on the earth's surface, would serve as an expression of the quantity of the centrifugal force of the corresponding parallel circle.

Thus, if a particle at A (fig. 1) were no longer subject to the earth's gravitation, it would

Fig. 1.



continue its motion in the direction of the tangent AM, and after the lapse of a second would arrive at M instead of at B. A, the point at which it was discharged, would meanwhile have reached B; and BM would denote for us the centrifugal force corresponding to the parallel circle ABD. Now in reality the action of gravitation never ceases, but is constantly directed to the centre of the earth, therefore at a certain angle to the direction of the centrifugal force BM. A freely displaceable particle of the surface would thus, under the action of the two forces, after the lapse of a second not be at M, but would slide on the surface of the earth to F, if there were no friction or other resistance. Every particle of water or air, being free to move, must thus have a tendency to recede from a particle (B) firmly adherent to the earth, and to approach the equator in the direction of the meridian. This tendency is expressed by the quantity BF, which is equal to $BM \cdot \sin \angle BMF$, or the centrifugal force of the parallel circle multiplied by the sine of the latitude. The centrifugal force

$$BM = C \cdot \cos \phi,$$

where C denotes the centrifugal force of the equator, and ϕ the latitude of the parallel circle. Consequently $BF = C \cdot \cos \phi \cdot \sin \phi$, and therefore reaches its maximum quantity when $\phi = 45^\circ$, amounting then to $\sin^2 45^\circ \times 0.11124 \text{ foot} = 0.05563 \text{ foot}$ in a second, or 4806.4 feet in 24 hours (which makes $1\frac{1}{3}$ verst, nearly $\frac{1}{3}$ of a German, or almost exactly $\frac{1.0}{1.1}$ of a British statute mile). This inconsiderable tendency toward the equator is further diminished by friction, and therefore cannot possibly be thought of as the motive force of a current. Yet it may perhaps contribute something to this—that in each of the oceans the current flowing, in the middle latitudes, from west to east gradually inclines in its direction a little to the equator.

The earth's centrifugal force, acting in an opposite direction to that of gravity, occasions over the entire earth, with the single exception of the two poles, a more or less perceptible diminution of weight. This is greatest at the equator, amounting to nearly the 290th part. Thence to the poles the quantity to be deducted from gravity diminishes in the ratio of the squares of the cosines of the latitudes. Now, as all bodies (air and water not excepted) are somewhat lighter in the vicinity of the equator than in higher latitudes, one would think that this must produce currents in the ocean and atmosphere equal to those arising from the lightening of the water and air by heating. These currents must flow beneath to the equator, and as upper currents from the equator to the poles. But in reality this does not appear to be the case; for degree-measurements and pendulum-observations have shown that the surface of the sea has the form of an ellipsoid slightly compressed at the poles, the long diameter of which (measured in the plane of the equator) is $\frac{1}{290}$ of its length greater than the shorter diameter (measured in the line of the earth's axis). From this we see that the level of the ocean at the equator is nearly as much raised as the weight loses there through the action of the centrifugal force; hence probably none, or a scarcely perceptible portion of the lighter water at the equator can flow off.

It is, perhaps, just the same with the atmosphere; yet it is probable that, with diminished pressure, the strong elasticity of the air will produce by expansion a greater raising of its level (if such an expression can be used) than the centrifugal force requires. If it is so, certainly the upper, much rarefied air must flow off from the equator, and be replaced by an undercurrent. Now, since the mass of the inflowing and of the outflowing air must be the same, the dense undercurrent will be considerably less perceptible than the upper strongly rarefied one. The centrifugal force may thus, combined with the difference of temperature, generate the currents in the upper strata

of the tropical atmosphere, and so also exert a certain influence on the trade-winds, but cannot possibly develop sufficient force to give rise to those winds.

We conclude the consideration of the influence of the earth's rotation on air- and sea-currents with the conviction that the existing explanations are altogether insufficient for the currents which flow parallel to the equator, because the rotation of the earth can only inconsiderably alter the direction of an already existing current, whether in air or sea, but can never independently produce a current of any importance.

C. ATTRACTION OF THE SUN AND MOON.

As is known, all the heavenly bodies attract each other, the force therein developed being, according to the law of the immortal Newton, directly as the mass and inversely as the square of the distance between the two bodies. If, therefore, we take as the unit of mass that of the earth, and as unit of distance the semidiameter of the earth, then, according to Newton's law, the force with which the sun attracts the earth's centre will be ex-

pressed by $\frac{319500}{(23400)^2}$. The moon attracts the centre of the earth

with the force $\frac{1}{80(60)^2}$ *. The ratio of the attractive force of the other heavenly bodies will be readily found in the same

manner. For example, the force with which Jupiter attracts the earth when nearest to it is one 25th part of that of the moon. The action of all the rest of the heavenly bodies, so vastly distant, is again considerably less.

Now, as the force with which a given heavenly body (the sun for instance) attracts the earth depends entirely upon the distance between the two, it is self-evident that the parts of the earth's surface which are nearer to the sun must be exposed to a greater attraction than those more distant. This can have no effect on the solid surface of the earth; but the easily displaceable particles of the sea and the atmosphere must have their equilibrium destroyed by its influence; and to restore the equilibrium currents must arise. In order to form true ideas of these currents, it is absolutely necessary to investigate more minutely the forces which call them forth, and the action of these forces. Before all things we must realize that we wish to con-

* We have assumed, after Klein (*Das Sonnensystem*), that the mass of the sun is 319500 times, and that of the moon one 80th part of that of the earth. For the mean distance, we have assumed that the distance of the sun is 350 times that of the moon, whose mean distance we estimate at 60 semidiameters of the earth.

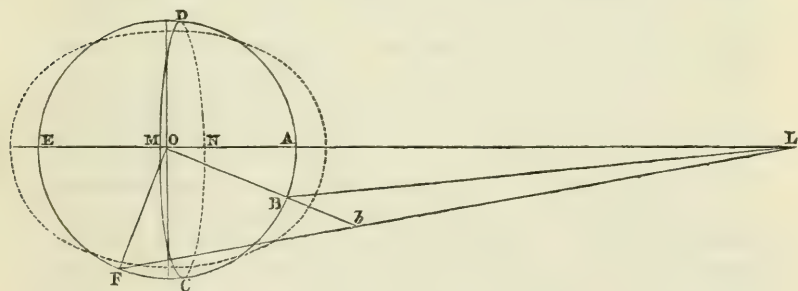
sider, not the absolute motion of each particle, but only the relative motion of the particles with respect to the earth's centre. Hence we have not to do with the whole of the attraction which any heavenly body exerts on the earth; the difference between the forces with which the earth's centre and the point of its surface to be considered are attracted gives the limits for our consideration. Thus, *e. g.*, the point which has the sun or the moon in the zenith is nearer to this heavenly body than the centre of the earth; and the difference of the attraction upon these two points might be expressed by $\frac{319500}{(23399)^2} - \frac{319500}{(23400)^2}$ for the sun, and by

$\frac{1}{80(59)^2} - \frac{1}{80(60)^2}$ for the moon. The second quantity is equal to about $2\frac{1}{2}$ times the first; and from this we infer that although the attraction of the sun is 168 times that of the moon, yet the difference between the attraction of a point at the surface and the centre of the earth by the moon is greater than the same by the sun; and therefore the effect produced by her attraction upon the currents of air and sea must also be greater.

For all other heavenly bodies this difference is so slight that we need not take it into consideration.

Supposing that the circle A C E D (fig. 2) represents the

Fig. 2.



earth, L the place of the centre of the moon or sun, and that k denotes the difference between the attraction of a point at the surface and the centre of the earth. The point A is attracted more strongly than the centre by the quantity k_1 . Now, as this attraction in the half of the earth turned towards the sun or moon acts in the opposite direction to the earth's attraction, $g - k_1$ will express the weight of any particle in the point A, the earth's gravitation being denoted by g . At the point B the difference between the attraction of it and the centre will be somewhat less. Let us call this difference k_2 ; then the weight at the point B may be expressed by $g - k_2 \cdot \cos LBb$; for here the

attraction of the moon acts at the angle LBb with the earth's gravitation. We thus see that the weight of the water and air becomes greater the further we remove from the point where the moon or the sun is in the zenith. In the points C and D, which are as distant as the centre of the earth, $k=0$; therefore the full attraction of the earth corresponds to the weight of the water and air in these points. In the other hemisphere, turned away from the moon, k is a negative quantity, because the centre of the earth is more powerfully attracted than any point of the surface of this hemisphere. But k acts in the same direction with gravitation, and must therefore be added to it in order to determine the weight of a particle in this hemisphere. The point E, most remote from the moon, is the most feebly attracted; hence in E also the quantity to be added to gravitation is the least, and the weight of a particle in E lighter than in any of the other points of the hemisphere which is turned away from the moon. In the point F, for example, the weight of a particle might be expressed by $g - k_3 \cdot \cos OFL$; and it would constantly diminish the more we approached the point E, which has the sun or the moon in the nadir, where its expression would be $g - k_4$. When the sun is in question, k_4 is only slightly less than k_1 ; but the difference is not insignificant when the attraction of the moon is taken into consideration; the difference between the attraction of the point which has the moon in the zenith and that of the centre of the earth is $1\frac{1}{2}$ the difference between the attractions of the centre and the point which has the moon in the nadir. Briefly, in each of the two hemispheres (one turned to the sun or moon, and the other turned from it) the minimum of the weight is found in the point of the surface which has the sun or moon in the zenith or nadir, but the maximum on the line D M C N, which divides the two hemispheres.

The pressure of the greater weight must cause a portion of the water and air to flow into the region where water and air are lighter; and hence a raising of the level will take place there, corresponding to the less weight, while in the region of the greatest weight the level will sink. Consequently both the sea and the atmosphere must endeavour to take the form of an ellipsoid the summits of which are on the line which passes through the centre of the earth and the moon. By the action of the sun, as by the moon's attraction, an ellipsoid somewhat less elongated will be formed in the sea and the atmosphere, its major axis being on the line which passes through the centre of the earth and the sun. In reality the actions of these two attractions will be combined and form only one tidal ellipsoid, which is most elongated when the actions of the sun and moon coincide—that is, at the times of full and new moon. On the contrary, the

raising of the level will be less when the major axes of the two ellipsoids are perpendicular to each other—that is, at the times of the first and last quarters of the moon.

All this we see confirmed in nature by the phenomena of ebb and flow of the tides. Many renowned mathematicians (among whom Newton, Euler, Laplace, and Airy occupy the first place) have endeavoured to determine by very ingenious mathematical calculations the laws of the tides; their theories, however, do not in all respects perfectly agree with the phenomena. We find, for instance, that on the coasts of the islands in mid-ocean the tide often rises only a few inches, and seldom amounts to more than from 2 to 3 feet, while one would think that it was just in the open ocean that the tide could be fully developed. According to the theory, the tide should assume the greatest dimensions in the tropical regions—instead of which, we find that, with very trifling exceptions, it is very moderate in the tropics, and does not reach, by a long way, the height it attains in the English Channel or on the coasts of the Bay of Fundy in Nova Scotia. Airy based his tide-theory on the theory of waves, and hence ascribes to the water-particles only a vertical oscillating motion. But, self-evidently, there cannot be anywhere an elevation of the sea-face, unless the necessary water flows to the place of elevation, water being incapable of elastic expansion; hence, among tidal phenomena, the existence of a horizontal motion of the water is undeniable. Nay, the horizontal motion must be very considerable, since it is able, in the course of a few hours, to call forth a not unimportant elevation of the water-surface over many thousands of square miles.

If the earth stood still and the same points had the sun or the moon in the zenith constantly, the surface of the sea would probably take the position of the tidal ellipsoid given by the theory, and always retain the same form. But now the relative position of the sun and moon to the earth is perpetually changing through the rotation of the latter; and therefore a very large volume of water and air must continually flow out of one part of the ocean into the other, in order to compensate the far-extended disturbance of equilibrium.

Now, as the relative change of place of the sun and the moon is very rapid, while for the complete formation of the tidal ellipsoid a certain time is necessary, it may be that the ellipsoid has not sufficient time to take its perfect form; the tendency, however, to form it must call forth currents in air and water, which will constantly follow the motions of the moon and the sun. If this be admitted, it explains to us why, in the open ocean, where these currents proceed undisturbed, no tide, or a very slight one, is observed; for only where insufficient depth or the shape of

the coast detains the current which follows the sun and the moon must the water more or less swell, and thereupon, in an undulatory motion, push the swelling further, in accordance with the law of the wave-theory, and in this manner carry the tide into high latitudes, whither, according to the theory of the moon's attraction, it should not come.

We are confirmed in this view of the tides by the circumstance that the tide-wave in the atmosphere has not yet been observed, although according to the theory it must show itself there more considerable than in the sea. The question of the atmospheric tide-wave has occupied many scientific men. Laplace, after a long series of observations, expressed a decided opinion that there is no atmospheric tide. More recently Bouvard, Eisenlohr, and Sabine have thought they could perceive a very small tide, expressing itself only in hundredth parts of a line on the barometer-scale.

By the way we must remark that the mercury of the barometer, just like all other bodies on the earth's surface, loses a portion of its weight by the attraction of the moon and sun; so that it cannot show the variation of the atmospheric pressure produced by the attraction of the moon, so long as the mass of air above it remains the same; every current, however, must alter the height of the mercury column. It is just the same with respect to the diminution of weight effected by the centrifugal force. An aneroid, as such, is not exposed to these influences, and therefore always gives the absolute pressure of the atmosphere; so that in principle it is preferable to the barometer. In practice, however, it still needs considerable improvements, because errors arise from the metal not possessing perfect elasticity. It is to be wished that observations of the two instruments were more frequently compared.

It was not until the present memoir had already appeared in Russian that I got a sight of the extremely interesting and instructive treatise on Tidal Phenomena by Dr. Schmick. This writing (which, while throwing much light on the phenomena of the tides, contends for many views to which we cannot assent) well deserves a closer consideration than would be in place here. Yet we cannot omit to say a few words on his notion of the displacement of the earth's centre of gravity. Since the height of the tide-wave is greater in the hemisphere turned towards the moon or sun than in the opposite one, Dr. Schmick thinks that the centre of gravity of the earth is displaced somewhat towards the side of the greater gathering of waters, and that the earth cannot by its own force recover its original centre of gravity after it has suffered displacement from without. A constantly repeated displacement, in this way, of the centre of gravity

in the direction of the southern hemisphere occasions there, according to Schmick, an accumulation of the waters. That the centre of gravity, displaced by external force, cannot of its own accord resume its former position is perfectly true; only Dr. Schmick seems to have forgotten that if the water rises higher on the hemisphere turned towards the moon than on the opposite, it is because it is lighter there on account of the greater attraction of the moon, and a greater gathering of the lighter water is necessary in order to restore equilibrium, without displacing the centre of gravity. If, therefore, the entire globe consisted of a liquid and had no rotation, the moon's attraction would cause it to take the form of an ellipsoid, of which the cusp directed to the moon would be somewhat higher than the cusp turned away from it; but the centre of gravity of the entire mass would remain undisturbed in its old place, because, as already said, the rise of the water on each point must be exactly equivalent to its loss of weight. As, however, the earth consists, for the most part, of a solid mass, which cannot alter its shape, and the hemisphere turned to the sun and moon loses more of its weight than the opposite one, the centre of gravity must be displaced in the opposite direction to that supposed by Dr. Schmick; namely, it must remove to a somewhat greater distance from those bodies. Of course the displacement is only very inconsiderable, even when the moon is at its least distance from the earth; yet it may to some extent favour temporary variations of the atmospheric pressure. In the moon, which constantly shows one side to the earth, the earth's attraction must thus cause the centre of gravity to lie permanently on the side which is turned away from us.

When the tidal wave does not attain its greatest height at the time required by the moon, or in consequence of collateral circumstances attains a far greater height than the moon's attraction demands (as in the Bay of Fundy, the English Channel, and many other places), the earth's centre of gravity will certainly remove temporarily in the direction of the elevation of the waters; but Schmick's view*, that the water must spread over the surface in accordance with the new centre of gravity as soon as the accumulating force ceases, cannot be regarded as correct, because any excessive accumulation of the water is followed by an equal sinking of the level. The earth's centre of gravity must follow these oscillations of the water, and hence, when this gradually comes to rest, will probably have returned to its old position.

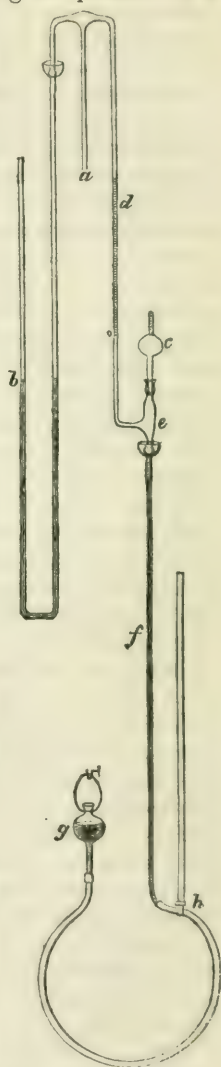
[To be continued.]

* *Fluth-Phänomene*, p. 128.

XVI. *Apparatus for Measurement of Low Pressures of Gas.* By Professor M'LEOD, *Indian Civil-Engineering College, Cooper's Hill*.*

THIS apparatus was devised for estimating the pressure of a gas when its tension is so low that the indications of the barometer cannot safely be relied on, unless indeed a very wide barometer and an accurate cathetometer be employed. The method consists in condensing a known volume of the gas into a smaller space and measuring its tension under the new conditions.

The form of the apparatus is the following:—The tube *a* communicates with the Sprengel, and with the apparatus to be exhausted; *b* is a siphon-barometer with a tube about 5 millimetres in diameter; and the principal parts of the measuring-apparatus consist of *c*, a globe of about 48 cubic centims. capacity with the volume-tube at the top, and *d* the pressure-tube; these two are exactly of the same diameter, to avoid error from capillarity. The tube at the bottom of the globe is ground into a funnel-shaped portion at the top of the wide tube *e*; and to the side of the latter the pressure-tube *d* is joined. The volume-tube at the top of the globe is graduated in millimetres from above downwards, the lowest division in this particular apparatus being 45; the pressure-tube *d* is also graduated in millimetres, the 0 being placed at the level of the 45th division on the volume-tube. A ball-and-socket joint connects the bottom of *e* with a vertical tube *f* about 800 millims. long, which is connected at its lower extremity by means of a flexible tube with the mercury-reservoir *g*; a stopcock at *h* permits the regulation of the flow of mercury into the apparatus: this may be conveniently turned by a rod, so that the operator may watch the rise of



* Read before the Physical Society, June 13, 1874. Communicated by the Society.

the mercury through a telescope and have the stopcock at the same time at command.

The volume-tube was calibrated in the usual way, by introducing weighed quantities of mercury into it, and making the necessary corrections for the meniscus. The capacity of the volume-tube, the globe, and upper part of the tube *e* was determined by inverting the apparatus and introducing mercury through *e* until the mercury flowed down the pressure-tube; the weight of this quantity of mercury, divided by the weight of that contained in the volume-tube, gives the ratio between the volumes; in the present case it is 1 to 54.495. While the apparatus is being exhausted, the reservoir *g* is lowered so as to prevent the mercury rising out of the tube *f*; but when it is desired to make a measurement of the pressure, the reservoir is raised and the mercury allowed to pass through the stopcock *h*. On the mercury rising into the tube *e* it cuts off the communication between the gas in the globe and that in the rest of the apparatus. Ultimately the whole of the gas in the globe is condensed into the volume-tube; and its tension is then found by measuring the difference of level between the columns of mercury in the volume- and pressure-tubes. On dividing this difference by the ratio between the capacities of the globe and volume-tube, a number is obtained which is approximately the original pressure of the gas; this number must now be added to the difference between the columns, since it is obvious that the column in the pressure-tube is depressed by the tension of the gas in the remaining part of the apparatus; on dividing this new number once more by the ratio between the volumes the exact original tension is found.

An example will best illustrate this. A quantity of gas was compressed into the volume-tube, and the flow of mercury was arrested when its surface reached the lowermost division on the tube. The volume was then $\frac{1}{54.495}$ of its original volume, and the difference between the levels of the mercury in the volume- and pressure-tubes was 66.9 millims.; this number, divided by 54.495, gives 1.228 as the approximate pressure. 1.2 must therefore be added to the observed column, which thus becomes 68.1; and on dividing by 54.495, the number 1.2497 is obtained as the actual pressure.

The relations existing between the contents of the other divisions of the volume-tube and the total contents of the globe were determined by measuring the tensions of the same quantity of gas when compressed into the different volumes. By this means the values of the divisions 40, 35, 30, 25, 20, 15, 10, 9, 8, 7, 6, 5, 4, 3, and 2 have been found; the experimenter is thus enabled to employ a division suitable to the quantity of gas

with which he has to deal. The smallest division contains only $\frac{1}{1492\cdot35}$ of the globe; consequently when a quantity of gas has been condensed into this space, its original tension will be multiplied 1492·35 times. In one case an amount of gas, which originally filled the globe, exhibited a pressure of only ·5 millim. when it had been compressed into the smallest division of the volume-tube; this indicates an original pressure of only ·00033 millim.

When measuring a tension, it is advisable to make two readings under different condensations, and to take the mean of the results. The following will give some notion of the precision attainable:—

I. At division 5 ·0225 } Mean ·0230.
 „ 2 ·0235 }

Remeasured.

At division 5 ·0228 } Mean ·0232.
 „ 2 ·0236 }

II. Barometer 0 millim.:—

At division 10 ·1985 } Mean ·1982.
 „ 5 ·1980 }

Remeasured.

At division 10 ·1953 } Mean ·1960.
 „ 5 ·1967 }

III. Barometer 0·6 millim.:—

At division 15 ·5488 } Mean ·5492.
 „ 10 ·5488 }
 „ 6 ·5501 }

Remeasured.

At division 15 ·5464 } Mean ·5469.
 „ 10 ·5464 }
 „ 6 ·5480 }

IV. Barometer 1 millim.:—

At division 20 1·2042 } Mean 1·2055.
 „ 15 1·2069 }

Remeasured.

At division 20 1·2082 } Mean 1·2090.
 „ 15 1·2099 }

V. Barometer 1·5 millim.: —

At division 30 1·9139 } Mean 1·9109.
 „ 25 1·9080 }

Remeasured.

At division 30 1·9041 } Mean 1·9040.
 „ 25 1·9039 }

VI. Barometer 2·1 millims.:—

At division 35	2·6017	} Mean 2·6045.
„ 30	2·6073	

Remeasured.

At division 35	2·6160	} Mean 2·6190.
„ 30	2·6220	

It may be mentioned incidentally that connexions for apparatus may be conveniently made by means of ball-and-socket joints of glass. The ball is made by thickening a piece of tube in the blowpipe-flame, and the socket by cutting in half a thick bulb blown on a glass tube. The ball is then ground into the socket by means of emery and solution of soda, and afterwards polished with rouge and soda solution. When slightly greased and with a small quantity of mercury in the cup, a joint is obtained which is perfectly air-tight and flexible*.

XVII. *On Wind-pressure in the Human Lungs during Performance on Wind Instruments.* By Dr. W. H. STONE†.

THE object of these experiments was originally physiological. It had been stated by many writers that the forced expiration employed in playing tended to produce emphysema of the lungs; but the real amount of such pressure had never been measured.

The facts elicited had also an interest of a purely physical character, which was the principal cause of their being brought before this Society, although, the writer of the paper remarked, it was on the border-ground between two great subjects of study that new phenomena were often to be looked for.

The experiments were two in number. The first aimed simply at measuring, by means of a water-gauge, the extreme pressure which could be supported by the muscles of the lips, both in trained musicians and in persons unaccustomed to the continuous exercise of these organs. The difference between different individuals was very great, some untrained persons having naturally considerable muscular power. About 6 feet of water was the ordinary maximum when a small tube was

* Since the above was written Dr. Sprengel has pointed out that Mr. Hartley (*Proc. Roy. Soc.* vol. xx. p. 141) has described as a “Sprengel joint” a connexion between two glass tubes made by grinding a conical tube into a conical cup and placing mercury or water in the cup. The difference between this and the one above mentioned is obvious: the former is quite rigid, the latter perfectly flexible.

† Read before the Physical Society, April 18, 1874. Communicated by the Society.

inserted between the lips. When the lips were supported by a cupped mouthpiece, such as is used for brass instruments, a greater height of the column could be obtained. The great majority of untrained persons could not support more than three or four feet of water. It was to be noticed that the lip-muscles invariably gave way long before the expiratory power of the thoracic muscles was exhausted. By pinching the lips round the orifice of the tube with the hand, and thus preventing their yielding, a far higher column of water could be supported.

The second experiment consisted in introducing a small bent glass tube into the angle of the mouth, connected with a flexible tube passing over the shoulder. It was found that most instruments could be played as well with this addition as without it. It obviously established a communication between the cavity of the performer's mouth, and therefore of his thorax, and the pressure-gauge. The following Table was compiled from many observations on some of our principal English musicians. The person experimented on was placed with his back to the gauge, the small tube was inserted in his mouth, and he was directed to sound in succession the chief notes of his instrument. As soon as the tone became full and steady, the position of the water-gauge was noted. A fair "mezzo-forte" note was employed. Of course, by forcing the wind and overblowing the instrument, much greater pressure could be obtained; but those given here were sufficient to produce an average orchestral tone.

Oboe . . .	lower notes	9 inches;	highest	17 inches.
Clarinet . .	"	15	"	8
Bassoon . .	"	12	"	24
Horn . . .	"	5	"	27
Coronet . .	"	10	"	34
Trumpet . .	"	12	"	33
Euphonium .	"	3	"	40
Bombardon .	"	3	"	36

It is to be noticed that the clarinet, in this as in some other respects, differs from its kindred instruments—and also that most of the pressures are small, not exceeding or, indeed, attaining the pressure of a fit of sneezing or of coughing. They are therefore very unlikely to injure the lungs, or to produce the emphysema erroneously attributed to them.

XVIII. *On the Fall in Pitch of Strained Wires through which a Galvanic Current is passing.* By Dr. W. H. STONE*.

THE object of this paper was to apply the vibrations of sound to the measurement of electrical currents, and to distinguish what was due to heating-effects from those caused by alteration of elasticity.

Strings of brass and steel, such as are used for pianofortes (No. 16 gauge), were stretched, by means of wrest-pins, across a resonant box, over bridges surmounted by brass bearings, and tuned to unison. On passing a current from two or more Grove's batteries through them, a very marked fall in pitch was obtained. The vibrating string being 24 inches long, and tuned to two-foot C, the tone sank above a fourth in steel and a major third in brass.

This result being a compound of actual lengthening by heat and of other causes, it was, in a second experiment, endeavoured to eliminate the former element by straining similar strings between the same bridges by means of a weight. This was attached to the arm of a bent lever, to the short end of which the string was made fast. By shifting the position of a four-pound weight along the arm, very accurate unison, or definite periodicity of beats could be obtained. When the current from the battery was passed through this string, free to expand by the falling of the weight, and therefore at a constant tension, a fall of pitch was still noticed. There was also a very marked loss of tone, which, on approaching a red heat, amounted to total extinction of sound.

A third experiment exhibited the changes of electrical resistance in a wire subjected to variations of strain. The wire was accurately balanced against another resistance in a Wheatstone's bridge, and the spot of light from a mirror-galvanometer joining the two circuits thrown on the screen. On suddenly increasing the tension and raising the musical pitch of the string, the galvanometer was visibly deflected. This was not an effect of heat (since the balance had been brought about during the passage of the current), and must be due to altered molecular state caused by the strain.

It was incidentally noticed that, when beats were produced by two strings on the same sonometer, they continued to be sensible to the touch by laying the hand on the instrument long after, from diminution of amplitude in the vibration, or from slowness in the beats themselves, they had ceased to be audible. This afforded a good demonstration of the continuity of sensation in touch and hearing.

* Read before the Physical Society, May 9, 1874. Communicated by the Society.

XIX. *On an Improvement in the Construction of the Spectroscope.*
By H. G. MADAN.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN the 'Proceedings of the Royal Society,' No. 152, p. 308, there is an account by Mr. Grubb of a very satisfactory method of correcting the curvature of the spectrum-lines, a defect inherent in all spectroscopes as at present made. This distortion is due, of course, to the fact that the rays from different parts of the slit fall on the prism under different vertical angles; and Mr. Grubb proposes to correct it by making the slit itself curved instead of straight, the distorting effect of the prism being then simply employed in rendering straight the slit-images which form the spectrum.

I think it just worth while to mention, in corroboration of Mr. Grubb's paper, that the same sufficiently obvious remedy occurred to me some time ago, and that since November last I have had curved slits in use for a lantern-spectroscope with perfectly satisfactory results. These are screwed on (in front of) the ordinary slit-plates, which latter are opened wide; and the curved plates are thus readily replaced by others by loosening a couple of milled-head screws. Any spectroscope may in this way have the additional slit-plates fitted to it with very little difficulty or expense.

I have two pairs of slit-plates with curved edges thus fitted to my original slit:—one with edges curved to a radius of 21 centims., which sensibly corrects the distortion of a single carbon-disulphide prism, the refracting angle of which is 60° ; the other slit has a radius of curvature of 10 centims., and is used with a train of two similar prisms. In using such slits with a condensing-lens between them and the prism, they should be so placed that the centre of curvature is on the side *towards* which the rays are refracted by the prism. The above curvatures were determined empirically by trials with tinfoil slits, which were easily made by attaching a piece of tinfoil to a plate of glass with gum, and (before the gum was dry) cutting out very narrow strips by a knife fixed to one leg of a pair of beam-compasses. In this way a number of trial slits may be made and tested; and when the curvature of that one which performs best is noted, any good optician will make a pair of brass plates with edges of the proper form.

I remain,

Yours faithfully,

H. G. MADAN.

XX. *On the General Theory of Duplex Telegraphy.*

By LOUIS SCHWENDLER*.

Introduction.

THE name of "duplex telegraphy" has been given to that mode of electric telegraphy which admits of the simultaneous transmission in opposite directions of signals between two stations through a single wire. That this name is far from happily chosen is evident; but as it is current and has already gained a recognized footing, it is not considered advisable to endeavour to replace it now by a more rational one, and it will therefore be adhered to throughout this paper†.

In the following investigation I shall endeavour to develop the mathematical theory of "duplex telegraphy" in its most general form, with the object of determining not only the best arrangement for any particular method, but also the relative values of different methods.

It is manifest that, having from general considerations decided on the best method, and further determined the best arrangement for this method, the remaining difficulties, due to the nature of the problem itself, will be exhibited in a clearer light, and the means of overcoming them may then be more easily discerned.

It is believed, however, that the sequel will show that, if the best method be adopted, and for this method the best arrangement be selected to suit the particular line on which the method is to be employed, the difficulties that stand in the way of duplex telegraphy will hardly be greater than those which are encountered every day in ordinary single telegraphy.

Imperfect Historical Sketch.

Having access to but scanty records in this country, I am not in a position to give an exhaustive history of this most important invention; and consequently the following sketch is necessarily incomplete, and must be taken as merely introductory, it being relegated to those better situated in this respect than myself to clear up the doubtful points of priority, and produce, what is much required, a complete history.

The idea of sending signals in opposite directions simultaneously through a single wire is by no means a new one. As

* From the 'Journal of the Asiatic Society of Bengal,' vol. xliii. pt. 2, 1874, having been read before the Society on the 4th of February, 1874. Communicated by the Author.

† The German language possesses a peculiarly suitable word in "*Gegensprechen*;" and the idea is fully rendered by "*gleichzeitiges Gegensprechen*."

early as 1849 Messrs. Siemens and Halske, of Berlin, took out a patent in England* for the simultaneous transmission of a plurality of messages by a suitable combination of wires; and although this patent does not refer directly to duplex telegraphy as it was subsequently understood, it must, notwithstanding, be regarded as a forerunner of it. In point of fact, Dr. W. Siemens's idea represents the general problem of which duplex telegraphy is only a particular case.

In 1854 Dr. Gintl, of Vienna, tried his "compensation" method of "duplex" working between that capital and Prague†, and on the 30th November of the same year read a paper before the Kaiserlich-königliche Academie of Sciences of Vienna‡ on the practical solution of the same problem by employing a Bain's electrochemical telegraph-apparatus instead of a Morse's receiving instrument.

In the summer of 1854, after Dr. Gintl's experiments between Vienna and Prague had brought the subject prominently to notice, Messrs. Siemens and Halske, of Berlin, and Herr Frischen independently, invented the "differential" method.

In January 1855 Edlund§ made experiments on the line between Stockholm and Gothenburg. He employed a "differential" method, which he had invented in 1848, for the purpose of measuring accurately Faraday's "extra currents."

In papers read at Paris on the 16th July and 6th August, 1855||, before the Academy of Sciences by M. Zantedeschi, he claims the honour of having first suggested the idea of duplex telegraphy; for as early as 1829 he had proved the possibility of the simultaneous transmission of currents in opposite directions through a single conductor. Having never seen his original communication of 1829, it is impossible for me to say how far these early ideas of Zantedeschi bear on the problem; but it is certain that both he and Dr. Gintl took a great deal of trouble to prove an erroneous theory, viz. that two distinct electrical currents can pass simultaneously in opposite directions through the same conductor without in any way interfering with each other. Such a supposition is in direct opposition to the electrical laws which were already known in 1829¶, and besides is in no way required in order to explain the simple phenomenon of duplex telegraphy**.

* 23rd October, 1849. The actual wording of the English patent is unknown to me.

† *Polyt. Centralbl.* 1853, p. 1475.

‡ *Wien. Akad. Sitzungsber.* xiv.

§ *Pogg. Ann.* 1856, vol. xcviii. p. 634.

|| *Ibid.* p. 123.

¶ Ohm published his classical work *Die galvanische Kette mathematisch bearbeitet* in the year 1828.

** Dr. W. Siemens, *Pogg. Ann.* vol. xcviii. p. 123.

None of the above methods, however, came to have extended, or indeed any, practical application. They appear to have been attempted doubtingly and without confidence; and although the trials are generally reported to have been successful, yet the methods were rejected as impracticable, and came to be regarded as merely of scientific interest*.

Only recently, after a torpid existence of almost twenty years, has duplex telegraphy been revived, and come to be the leading topic in telegraphy, securing after such a lapse of time the amount of public interest it rightly deserves.

To Mr. Stearns, an American telegraph-engineer, is due the honour of having appreciated the real value of duplex telegraphy, and of having (by giving the system, modified by improvements of his own, an extended application on the lines of the United States) proved its thorough practicability.

Inquiry into the Causes which have delayed the introduction of the System.

When Steinheil in 1837 announced his discovery of the feasibility of employing the earth to complete the electric circuit instead of a return-wire, telegraph-engineers immediately recognized its immense mercantile value, and did not delay to verify his results.

Now, in the career of telegraphy, the invention of duplex working ranks second only in importance to Steinheil's discovery. The utilization of the earth reduced by one half the number of wires required to carry a given traffic: duplex telegraphy again almost halves this number. In the face of this fact it is not easy to understand why the one idea received immediate and universal application, while the other, of only about ten years more recent date, has met, until now, with universal neglect; but on closer examination it will be found that there have been perfectly comprehensible, although not all rational, influences at work.

An inquiry into the circumstances, therefore, that have caused the discovery of a system, the introduction of which must mark the second great era in telegraphy, to lie fallow for nearly twenty years is of the utmost interest, and cannot fail to be instructive with regard to the prospects of future progress.

From an examination of the methods originally proposed for duplex working, it will be found that they do not in any way essentially differ from those which may now come into actual use. The causes, therefore, which have prevented the introduction of the system must be sought for external to the methods.

* For the light in which duplex telegraphy was regarded till quite lately, see Schellen, Dub, Sabine, Blavier, Kuhn, &c.

The first of these, we find, is that the invention was in advance of the requirements of the age. Telegraph-lines had already been constructed which were quite capable of carrying the given traffic and even more. Further, any increase in traffic could be easily met by an increase in the number of wires on the existing telegraph-posts, instead of by resorting to a system which had a complex appearance, and after all might not answer.

However, although the above considerations explain the course of events in certain limited instances, and up to a certain time, they do nothing towards justifying the costly expedients that have been generally adopted until recently in preference to introducing duplex telegraphy—for instance, the reconstruction and multiplying of long overland lines, and especially the laying of a second submarine cable when the traffic became too great for one.

It is true that the successful application of any duplex method requires lines of a more constant electrical condition, receiving-instruments of a larger range*, and telegraph-operators of a somewhat better professional education; but surely these three conditions have not all *at once* become fulfilled (since 1872), so as to make duplex telegraphy possible only just now? No; the causes which have delayed its introduction so long have been of a much less technical and more irrational nature.

The mere fact of the duplex methods appearing complex prevented telegraph-administrations from thinking seriously of introducing them. The ingenious methods were never tried with that zeal and perseverance which is necessary to carry a new invention successfully through. They were indiscriminately rejected after a few trials made without method or consideration; and the real conditions of success or failure were never examined or pointed out. Thus naturally a prejudice was created against duplex telegraphy, and it was fostered by a host of school literature up to the latest time, as pointed out before. Further, not a single physicist or electrician investigated the question with a view to ascertaining what quantitative effect the variable condition of lines has on duplex working as compared with single working.

If such an investigation had been made, it would have been found that the technical obstructions in the way were by no means so formidable as had been represented, and that the

* By the "range" of a telegraph-instrument I understand the ratio of the largest to the smallest force by which the instrument in question can be worked without requiring a fresh mechanical adjustment. For instance, Siemens's beautiful relays can be easily adjusted to a range of 20; *i. e.* they can be made to work with one cell through an external resistance equal to their own resistance, and with ten cells through no external resistance, *without* giving the tongue a fresh adjustment.

electrical condition of the lines, as well as the perfection of the instruments and the professional education of the staff, would have fully admitted of the successful introduction of duplex telegraphy at least ten, if not twenty, years ago.

It is true indeed that the suggestion of using condensers for balancing the charge and discharge of a line has only been made very lately, being one of Stearns's happy ideas ; but this should have been no reason against introducing the system on short and overworked lines, where the charge and discharge is imperceptible. If only one telegraph-administration had shown the perfect practicability of the system on a short line, the cloud of prejudice would have been dissipated, and suggestions for overcoming the charge and discharge on long overland lines and submarine cables would have been readily enough given, and thereby large capitals saved.

To sum up, therefore, we have the following causes which acted persistently against the introduction of duplex telegraphy.

First, the invention was in advance of the age.

Secondly, the telegraph profession, young as it is, is far more conservative than is good for the advance of telegraphy ; and, on the whole, telegraph-administrations and staffs have by no means that professional education which is required to conduct practical experiments with a clear understanding, and thence deduce rational conclusions. Thus prejudice was created, which was increased from year to year by authors of school literature writing most discouragingly on the subject.

Thirdly, unfortunately during all that time no physicist found it worth his while to investigate the duplex methods with a view to ascertain quantitatively what can be expected of them, and how they actually compare, with respect to safety, with single working.

Fourthly, duplex working itself could not progress, because it was neither tried nor investigated, and hence no suggestions for overcoming the difficulty of charge and discharge were called for.

Great honour must therefore be given to Mr. Stearns, who brought up the subject again so prominently, and who by his zeal succeeded in introducing it on a large scale, and so elevated the ingenious methods from the questionable position of "interesting scientific experiments."

I think far less of his idea of introducing condensers or Ruhmkorff's coils to balance the charge and discharge of lines, than of his having taken the neglected child up again against the prejudice of his own profession, and shown that it could have a healthy existence even in the backwoods of America. I trust that these remarks will not be considered irrelevant in

the present investigation, since they tend to show how real progress in one of the youngest branches of applied science may be retarded for a considerable period by nothing but prejudice of the profession themselves, for whom the progress should be the first essential; and administrations will see how much the advance of telegraphy will always depend on their recognizing and encouraging by experiment inventions that are theoretically sound and tend in the right direction.

General Considerations.

Before entering on the solution of the problem for any particular duplex method, it would be advisable once for all to state definitely the nature of the general question before us. This will not only save time, but the subsequent special solutions can then also be made under a general guide; and thus, being well linked together, the whole investigation will become far more lucid and concise than it otherwise would be.

While in ordinary (single) telegraphy the signals are always produced in the same way, *i. e.* by the signalling current arriving through the line from the distant station, the signals in duplex telegraphy may be produced in either of two ways essentially different from each other. Namely, if the times of sending from the two stations fall together, *i. e.* no current, or double current, or any difference of currents is in the line, the signals, so long as this state of the line exists, are produced wholly or partly by the battery of the receiving-station. Signals produced in this way we shall call "duplex signals;" and these signals alone indicate the essential difference between duplex and ordinary telegraphy.

If, however, the moments of sending from the two stations do not fall together, the signals are then produced as in ordinary telegraphy, and may be appropriately designated "single signals."

It will be clear, then, that when the two stations are at work at the same time, "duplex signals" and "single signals" must necessarily follow each other in accidental succession. Nay, one and the same signal produced in either station may be partly a "duplex" and partly a "single" signal.

To secure, therefore, regularity of working, the signals produced in either way should be invariably of equal strength.

Further, as in duplex telegraphy the receiving-instruments must be always permanently connected up with the line, it is one of the first requirements that the out-going or sent current from any station should in itself have no effect whatever on the receiving-instrument of *that* station, in order that the instrument may be entirely free to receive signals from the distant

and if P cannot be always kept rigidly equal to Q , we should at least have

$$P - Q = S \text{ as small as possible, } \dots \dots (IV.)$$

μ , P , and Q being functions of the resistances and electromotive forces of the system, which are known so soon as the particular duplex method has been selected.

The general problem which is to be solved for duplex telegraphy may now be clearly stated as follows:—

D and S are two known functions which must be rigidly equal to zero when no variation in the system occurs, and which for any given variation in the system must be as small as possible, and approximate rapidly towards zero as the variation in the system becomes smaller and smaller.

Thus the solution of the problem for any given duplex method will always be a question of the minima and maxima calculus.

Having then ascertained the best arrangement for each duplex method, the methods can be compared *inter se*; and that method will be best, and should be selected for use, which for any given variation in the system gives the least absolute magnitude to the functions D and S .

If we suppose, however, that the particular duplex method is not given, the problem to be solved becomes more general, but would still be entirely within the limits of the variation calculus, furnishing, no doubt, a very interesting and important application of that most powerful mathematical instrument. The general solution would at once determine the best method possible, after which special solutions would give the best arrangement for that best method.

It is, however, not my intention to endeavour to solve here the duplex problem in this most general form. To be able to indicate so general and desirable a solution is by no means identical with being able to effect it. The task before me is far more simple, since, as already pointed out, I shall investigate each duplex method separately to determine its best quantitative arrangement, and ultimately compare the different methods to ascertain their relative values.

To do this, the question may be attacked in two different ways, depending on the purpose for which the solution is required.

Namely, either the solution is to be made when considering the line as a variable conductor only, but not acting perceptibly as a Leyden jar; or the line is to be considered as constant in conduction and insulation, but acting as a Leyden jar of large capacity. In the first case the solution would be directly applicable to short overland lines (not over 200 miles in length), and in the second case to submarine cables, which, if good, may

always be considered sensibly constant in conduction and insulation.

Further, as a long overland line acts both as a variable conductor and as a Leyden jar of sufficiently large capacity, it would then be necessary to give a solution with respect to both these effects. To obtain, however, the same result without rendering the problem too intricate, it will be best to separate the two questions from the beginning, and afterwards combine their solutions judiciously for application to the case of overland lines.

1st PROBLEM. *What is the best arrangement of any given duplex method when the line is regarded as a variable conductor, but not as acting perceptibly as a Leyden jar?*

2nd PROBLEM. *What is the best arrangement of any given duplex method when the line is regarded as a Leyden jar of large capacity, but not as a variable conductor.*

The second problem may be expressed more clearly as follows:—

2nd PROBLEM. *What must be the distribution of condensers along a given resistance, in order that the two essential conditions (I. and II.) may be least disturbed for a speed of signalling variable between two fixed limits?**

* A telegraph-line always acts as a condenser with capacity and conduction-resistance in each point of its entire length, while an artificial condenser (such as a Leyden jar) which we are able to produce sufficiently cheaply has only capacity but no perceptible conduction-resistance in each point. This is in fact the essential difference between a line and a condenser; and therefore, in order to render their charges and discharges under the same circumstances as nearly as possible equal, as is required for duplex working, it will be necessary to find the law according to which to distribute a certain given system of condensers along a given resistance.

This law will clearly be a function of the signalling speed within its limits of variation. For instance, say the signalling speed is constant, or its range zero, then clearly one condenser connected to any point of the given resistance would suffice; only the magnitude of the capacity of this one condenser would be determined by its position with respect to the resistance, and, in addition to this, would of course be fixed by the signalling speed and the known capacity of the line.

Further, say the speed of signalling is variable between 0 and ∞ , or its range is infinite, then clearly only an infinite number of small condensers distributed along the given resistance in the very same manner as the capacity is distributed along the line would strictly answer the purpose; in fact, the condenser required in this imaginary case would be nothing more or less than a second telegraph-line, identical with the one used for signalling. In practice, however, the speed of signalling varies only between narrow limits; and therefore the number of condensers required to reproduce as nearly as possible the action of the line with respect to charge and discharge, will become few, especially if the best system of distribution has been determined. Until this law is known, we can do nothing

It is clear that the nature of these two problems is very different, because in the first we have to deal with forces constant with respect to time, while in the second the forces acting are functions of time, *i. e.* of the signalling speed. (The forces in this case are proportioned to the *true currents*.) The latter problem being far the more intricate, and for my special purpose only of secondary importance, I shall begin with the solution of the first.

Solution of the first Problem for any given Duplex Method.

What is the best arrangement of any given duplex method when the line is regarded as a variable conductor, but not as acting perceptibly as a Leyden jar?

I. The bridge method*.

This arrangement for duplex working is based on the well-known method of comparing electrical resistances, "Wheatstone's bridge;" and the figure (p. 127) gives the general diagram when this method is applied for duplex working.

β is the internal resistance of the signalling-battery.

L' the "measured conductor"† resistance of the line when measured from station I;

$$\therefore L' = l' + \frac{i l''}{i + l''}.$$

but find it approximately by experiment, however tedious it may be to do so.

It has also been proposed to use Ruhmkorff's coils for balancing the effect of charge and discharge. This method, however, I believe must be always much inferior to the one of using condensers, inasmuch as the strength of a voltaic induction-current scarcely depends on the speed of signalling, while the charge and discharge of a line, it is well known, is not at all an inconsiderable function of the signalling speed.

Therefore if the strength of the induction-current had been adjusted to balance the charge and discharge of the line for a certain signalling speed, the balance would be considerably and at once disturbed if the speed varied even slightly; and since so long as hand signalling is used a certain variation in the speed of signalling will always exist, this method will prove a failure, or at all events will render fresh adjustments more frequently necessary than when condensers are used.

* Dr. W. Siemens mentions this in Pogg. *Ann.* vol. xcviii. p. 122 (1856).

Mr. O. Heaviside (Phil. Mag. 1873, vol. xlv.) states that Mr. Eden, of Edinburgh, claims to have suggested this method at about the same time as Mr. Stearns, of Boston, U.S.A., took out a patent for it.

† Generally these measured values L' and L'' will be different from each other, especially for long overland lines. They can become equal only under two conditions—either if the resistance of the resultant fault (i) is so great that the total conductor resistance of the line ($l' + l'' = l$) can be neglected against it, or for any magnitude of i if the latter has a position in the middle of the conductor, *i. e.* when $l' = l'' = \frac{l}{2}$.

L'' the "measured conductor" resistance of the line when measured from station II;

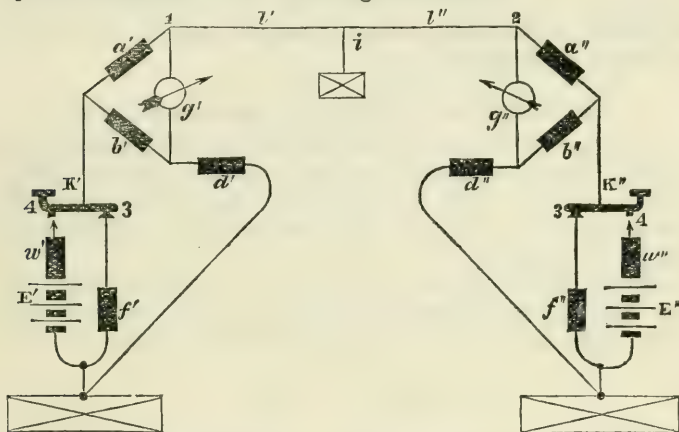
$$\therefore L'' = l''' + \frac{il'}{i + l'}.$$

ρ' the complex resistance of the duplex arrangement in station I., *i. e.* the resistance between point 1 and earth.

ρ'' the complex resistance of the duplex arrangement in station II., *i. e.* the resistance between point 2 and earth.

E , electromotive force of the signalling-battery.

g , the resistance of the receiving-instrument.



K , telegraph-key of peculiar construction, to be described hereafter.

g , the receiving-instrument connected up in that branch of the bridge which, when measuring resistances, would contain the galvanometer*.

a , b , and d are the branches of the bridge.

f , the resistance between the rest-contact of the key and earth.

w , an additional resistance to be inserted in the battery-branch, for reasons to be given further on.

i , the resistance of the resultant fault ("real absolute insulation" of the line) acting at a distance l' from station I, and at a distance l'' from station II. (both l' and l'' expressed in resistances so that $l' + l'' = l$ equal the "real conductor resistance" of the line).

To be quite general, we must suppose that the telegraph-line which connects the two stations I. and II. has a different resistance when measured from station I. than when measured from station II., and that therefore the best resistance-arrangement of station I. must be also different from that of station II. with respect to magnitude of resistances.

* Siemens's polarized relays are well adapted for this purpose, on account of their great sensitiveness and wide range; D'Arincourt's relays would also answer well.

The resistances which are similarly situated in both the stations will be designated by the same letters; and to indicate the station to which they belong, each letter will have *one* accent in station I. and *two* accents in station II.

Further, if a relation between the resistances of one station has to hold good between those of the other station also, the letters will be used without any accents.

The great practical advantage of the bridge method, it will be clear at once, is that any kind of receiving-instrument which has been used for single working may also be employed for duplex telegraphy. This fact must always be of great consideration for any administration that contemplates the general introduction of duplex telegraphy.

General expressions for the two functions D and S.

To obtain the functions D and S, we have first to develop the general expressions for the forces p , P , and Q , say for station I.

By p' we understand the force which acts on the receiving-instrument g' of station I. when that station is sending alone (station II. at rest).

p' , in our particular case, is therefore proportional to the current which passes through the galvanometer in a Wheatstone's bridge when balance is not rigidly established; thus

$$p' \propto E' \frac{\Delta'}{N'}$$

where

$$\Delta' = a'd' - b'(I' + \rho'') = a'd' - b'c',$$

and

$$N' = g'(b' + d')(a' + c') + f' \{ g'(a' + b' + c' + d') + (c' + d')(a' + b') \} \\ + a'c'(b' + d') + b'd'(a' + c').$$

Further, by P' is understood the force which acts on the receiving-instrument in station I. when station II. is sending alone: *single signals*.

This force in our particular case is proportional to the current which passes through the receiving-instrument of station I. when station II. is sending alone; and we have consequently

$$P' \propto C''\mu'\psi',$$

where C'' is the current which enters the line at point 2 when station II. alone is sending, $C''\mu'$ the part of this current C'' which arrives actually at point 1 (on account of leakage between points 2 and 1, a part of C' is lost), and $C''\mu'\psi'$ that part of the current $C''\mu'$ which ultimately produces the signal (*single signal*) in station I. The current $C''\mu'$ arriving at point 1 branches off

in two; one part goes through a' and the other through g' to earth.

Further,

$$C'' = E'' \frac{m''}{N''},$$

$$\therefore P' \propto E'' \frac{m''}{N''} \mu' \psi';$$

where

$$m'' = g''(b'' + d'') + d''(a'' + b''),$$

$$\mu' = \frac{i}{i + l' + \rho'},$$

$$\psi' = \frac{f'(a' + b') + a'(b' + d')}{(f' + d')(a' + b' + g') + b'(a' + g')},$$

and N'' an expression identical in form with N' .

Further, by Q' we understand the force which acts on the receiving-instrument of station I. when both stations are sending simultaneously: *duplex signals*.

This force is again proportional to the current which, under these circumstances, passes through the receiving-instrument g' of station I.

This current can be expressed by

$$E' \frac{b'}{n'} - \sigma' \phi';$$

and therefore

$$Q' \propto E' \frac{b'}{n'} - \sigma' \phi',$$

σ' being the current actually in the line at point I when both stations are sending simultaneously; and this current, being the algebraical sum of two currents, may be either +, 0, or -. We will suppose that σ' contains the sign itself.

Further, we have

$$\sigma' = \frac{E' m'}{N'} - \frac{E'' m''}{N''} \mu',$$

$$n' = (b' + d' + f')(a' + g') + b'(f' + d');$$

and ϕ' is a function which becomes identical with ψ' if we put

$$w' + \beta' = f'.$$

Therefore the two functions D and S are for the bridge method (station I.) most generally expressed as follows:—

$$D' = \frac{E'}{E'} \frac{N''}{N'} \cdot \frac{1}{\mu'} \frac{\Delta'}{m'' \psi'} \quad . \quad . \quad . \quad . \quad . \quad (III').$$

and

$$S' = E'' \frac{m''}{N''} \mu' \psi' - \frac{E' b'}{n'} + \sigma' \phi'; \quad . \quad . \quad (IV'.)$$

and similar expressions will be obtained for station II., namely

$$D'' = \frac{E''}{E'} \frac{N'}{N''} \cdot \frac{1}{\mu''} \frac{\Delta''}{m' \psi''} \quad . \quad . \quad . \quad (III'')$$

and

$$S'' = E' \frac{m'}{N'} \mu'' \psi'' - \frac{E'' b''}{n''} + \sigma'' \phi''. \quad . \quad . \quad (IV'')$$

Rigid fulfilment of the first condition, i. e. $D=0$.

For station I. we have

$$D' = 0,$$

which equation can only be satisfied by

$$\Delta' = 0,$$

since the other factor of D' cannot become zero for quantities larger than 0 or smaller than ∞ . Then, substituting for Δ' its value, we have

$$a'd' - b'(L' + \rho'') = 0; \quad . \quad . \quad . \quad (V.)$$

or balance in station I., when that station is sending and station II. is at rest, must be rigidly established.

Therefore if balance in station I. is disturbed, say by L' varying or by any other cause* external to L' , we must have means of conveniently reestablishing balance without delay. This, of course, could always be done by altering either all the branches a' , b' , and d' , or any two of them, or only one of them; but it is clear that so long as the variation of L' which disturbs the balance does not exceed certain limits, balance may be regained by altering only *one* of the three branches available; and as this will also be more convenient in practice than altering two of the branches, or all three simultaneously, we shall make the supposition that

“Balance is reestablished by an appropriate readjustment of one of the three available branches”†.

* Causes of disturbance to balance external to L' are inappreciable in practice and therefore may be neglected from the beginning.

† Finally, when the best resistance-arrangement has been found, the resistance of the different branches will be expressed in terms of L ; and therefore to keep the best arrangement when L varies between any two given limits will involve necessarily a simultaneous alteration of the resistance of all the branches.

If, however, the variation of L is small in comparison with L itself, an alteration of *one* branch for the purpose of reestablishing balance is justified, and would be absolutely correct if the variation of L were infinitesimal.

The question therefore is, which of the three branches, a , b , or d , is the best adapted for the purpose?

To decide this we must remember that for station II., in accordance with the first condition ($D=0$), a similar equation has to be fulfilled, namely,

$$a''d'' - b''(I'' + \rho') = 0. \quad \dots \dots (V'')$$

Now ρ' , the complex resistance of the arrangement in station I., is a function of all the resistances in station I.; and similarly ρ'' , the complex resistance of the arrangement in station II., is a function of all the resistances in station II. Therefore, generally, if in order to obtain balance, say in station I., any of the three branches a' , b' , d' were adjusted, ρ' would alter in consequence of this readjustment, and thereby the balance in station II. (equation V'') would be disturbed, and *vice versa*. In other words, the readjusting in one station would interfere with the balance in the other station, and therefore rigid balance could be only attained after a series of successive adjustments in both the stations—and then only, from a theoretical point of view, approximately, introducing practical difficulties almost insurmountable.

However, examining the positions of the three branches, it will be seen at once that b acts as the galvanometer-branch of a bridge for any current arriving through the line. Thus, if we were to fulfil the condition

$$ad - fg = 0 \quad \dots \dots (VI.)$$

for both stations, the value of ρ would become at once independent of b^* , and consequently any adjustment of b' to reestablish balance in station I. would not affect in the slightest degree the balance in station II., and *vice versa*.

Thus, presupposing the fulfilment of this condition (equation VI.) for both the stations, the branch b would evidently be the best suited for adjustment†. Under these circumstances it would then be clear that balance in either station can be obtained by a *single* adjustment of b ; and therefore we may call equation VI. “*the immediate-balance condition* ;” and the fulfilment of this condition being of the greatest practical importance to ensure the success of duplex working, we are justified, nay even com-

$$* \quad \rho = \frac{(g+d)(a+f)}{a+d+f+g} - \frac{(ad-fg)^2}{F(b)}.$$

Therefore if $ad - fg$ is very near zero, ρ becomes most rapidly independent of b .

† Further, it must be remarked that, even if the condition $ad - fg = 0$ be not rigidly fulfilled, still by adjusting in the branch b we have “*accelerated*” balance, whereas by adjusting in a or d we should, on the contrary, have “*retarded*” balance.

pelled, to use this relation (equation VI.) as the basis for all subsequent investigations.

We will therefore suppose henceforth that

$$ad - fg = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (VI.)$$

is rigidly fulfilled for both the stations.

But as the value of f depends on the position of the key, which during signalling moves from contact 3 to contact 4 and back, the rigid fulfilment of equation (VI.) necessitates at once that

$$w + \beta = f, \quad . \quad . \quad . \quad . \quad . \quad . \quad (VII.)$$

not only for both the contacts 3 and 4, but also for all the intermediate positions of the key. Thus, supposing that $w + \beta = f$, *i. e.* the resistance from contact 4 through battery to earth equal to the resistance from contact 3 to earth, a key constructed in such a way that contact 4 is not broken before contact 3 is made, and that contact 3 is not broken before contact 4 is made, would fulfil the required condition entirely. Keys of this kind can be easily enough constructed. It is true that in any such key there will be always a moment when the contacts 3 and 4 are simultaneous, and when therefore the resistance to earth is not f , as

it ought to be, but only $\frac{f}{2}$. Considering, however, that the time during which this error lasts is very small compared with the time it takes to make a signal, its disturbing effect will never be appreciable in practice; *i. e.* ρ will remain sensibly constant during the time the key is moved to produce a signal.

There will be no practical difficulties connected with the fulfilment of equation (VII.), and therefore also none with the fulfilment of equation (VI.); for β , the internal resistance of the signalling-battery is the only quantity which of itself can alter in time. However, this variation of β for any efficient form of signalling-battery being invariably steady and small, it will be always possible to neutralize its action in time by a simple readjustment of w .

If Leclanché's cells are used, or well prepared Minotti's, a weekly adjustment of w should be sufficient. The measuring of β will always be an easy matter*.

* My friend Mr. R. S. Brough suggested the following very simple method for keeping

$$w + \beta = f \dots\dots\dots (VII.)$$

Insert a small galvanoscope in the branch b , for which balance is established with respect to the received current, i , e .

$$ad - fg = 0, \dots\dots\dots (VI.)$$

Now note the deflection on the galvanoscope when both stations are

Rigid fulfilment of the second condition, i. e. $S=0$.

The general expression for S' was

$$S' = \frac{E''m''}{N''} \mu' \psi' - \frac{E'b'}{n'} + \sigma' \phi'. \quad \dots (IV').$$

Remembering that by equation (VII.)

$$w' + \beta' = f',$$

we know that $\psi' = \phi'$; and substituting further for σ' its value, the general expression for S' becomes

$$S' = \frac{E''m''}{N''} \mu' \psi' - \frac{E'b'}{n'} + \left\{ \frac{E'm'}{N'} - \frac{E''m''}{N''} \mu' \right\} \psi'; \quad (IV').$$

and this form of S' shows at once that it is perfectly immaterial for duplex working by the bridge method whether the same or opposite poles of the two signalling-batteries be put to line*; for in both cases equation (IV'.) becomes

$$S' = \frac{E'm'}{N'} \psi' - E' \frac{b'}{n'}. \quad \dots (IV').$$

Further, it will be seen that the right-hand member of equation (IV'.) can be transformed† into $E' \frac{\Delta'}{N'}$, which is equal to p' , or we have generally

$$S = p;$$

i. e. *the difference of forces by which duplex and single signals in*

sending simultaneously, and again when the station for which β is to be measured is sending *alone*. Then clearly, if these two deflections are equal, $w + \beta$ must be equal to f . If the two deflections are not equal, then alter w until they become equal. After the determination is made the galvanoscope is short-circuited.

* In practice, however, I prefer to put the same (namely the positive) poles to the line, as then defective insulation will not be felt so much.

† We have

$$\begin{aligned} \psi &= \frac{k}{n}, \\ N &= \frac{mk - \Delta n}{b}, \\ \therefore S &= \frac{Eb\Delta}{mk - \Delta n} \\ &= \frac{Eb\Delta}{bN} \\ &= \frac{E\Delta}{N} = p. \end{aligned}$$

the same station are produced is equal in magnitude and sign to the force by which balance in that station is disturbed.

Consequently the rigid fulfilment of the first condition ($D=0$) will entail the rigid fulfilment of the second condition ($S=0$); and this, it will be clear, is only due to the fact that the complex resistance ρ is independent of b , and that the key during signalling does not alter ρ ; whence it follows that the perfection of the key in this respect is of the greatest importance. There are, however, no practical difficulties connected with the construction of a key which fulfils condition (VII.) perfectly.

By the aid of the relations given in equations (VI.) and (VII.) we have therefore gained the great practical advantage that duplex telegraphy will be entirely on a par with single telegraphy, if the means of attaining rigid balance are sufficiently accurate, convenient, and rapid.

But, even supposing that we are unable to keep that balance rigidly for any length of time (on account of L varying), we can nevertheless bring the regularity of duplex working as near as possible to that of single working by making D and S as small as possible for any given variation of L .

Rapid approximation of the two functions D and S towards zero.

For station I. we had

$$S' = p' \propto \frac{E'm'}{N'} \psi' - \frac{E'b'}{n'}, \quad \quad (IV'.)$$

which we may also write

$$S' = p' \propto \frac{E'b'}{n'} \left\{ \frac{1}{1 - \frac{\Delta'}{m'\psi'}} - 1 \right\}, \quad . . . \quad (IV'.)$$

since

$$\frac{m'}{N'} = \frac{b'}{k' - \Delta' \frac{n'}{m'}}$$

and

$$\psi' = \frac{k'}{n'}.$$

Further, if we call b' the value of b which in station I. establishes rigid balance for any given values a' , d' , and L' , we have

$$\Delta' = b' \cdot \delta I',$$

where $\delta I'$ is the variation of I' which throws the balance out, and which variation may be either positive, zero, or negative ($\delta I'$ shall contain the sign in itself).

Further, substituting

$$\frac{m'\psi'}{b'} = y'$$

and

$$\frac{E'b'}{n'} = G',$$

the expression for S' may be written as follows:—

$$S' = p' \propto G' \left\{ \frac{\frac{F'}{1 - \frac{\delta L'}{y'}} - 1}{1 - \frac{\delta L'}{y'}} \right\} = G' F',$$

which is the best form of S' for our purpose.

The function S' consists of two factors—namely, of G' , which at or near balance is proportional to the current by which duplex and single signals in station I. are produced, and of F' , which at balance = 0.

Therefore to make S' as small as possible when balance is disturbed, we can only do so by making F' as small as possible, which is evidently the case for $y' = \frac{m'\psi'}{b'}$ a maximum. Further,

$$D' = \frac{p'}{P'} = \frac{S'}{P'},$$

$$S' = G' F';$$

and since at or near balance

$$P' \propto G',$$

it follows that

$$D' = F';$$

i. e. the first condition is also fulfilled by

$$y' = \frac{m'\psi'}{b'} \text{ a maximum.}$$

Our problem for station I. would therefore be most generally solved if we make the function y' a maximum, remembering that the variables contained in y' have to fulfil two condition equations, namely the *immediate balance* (equation VI.) and the *balance* (equation V.).

Substituting for m' its value, and remembering that

$$\psi' = \frac{a'}{a' + g'},$$

on account of the *immediate-balance* condition (equation IV.), we get

$$y' = \frac{a'(g' + d'')}{a' + g'} + \frac{a'd'}{b'}.$$

From the development of these general results it will be evident that they fulfil the following conditions:—

I. *The irregularity of signals in the one station is entirely independent of the irregularity of signals in the other station.*

II. *The irregularity of signals in each station is due only to balance not being rigidly established.*

III. *If balance in either station is disturbed, a single adjustment in the branch b will reestablish that balance.*

IV. *Any disturbance of balance will have the least possible effect on the received signals.*

V. *Maximum current at balance.*

VI. *Maximum magnetic effect of the maximum current on the receiving-instrument.*

[To be continued.]

XXI. *On a simple Arrangement by which the Coloured Rings of Uniaxial and Biaxial Crystals may be shown in a common Microscope.* By Dr. W. H. STONE*.

THE author was not aware that any arrangement had been hitherto supplied to the ordinary microscope other than an extra top to the eyepiece containing a supplementary stage and an analyzer. This could only be considered a clumsy expedient.

The objects to be obtained were clearly two:—first, to transmit the rays at considerable obliquity through the plate of crystal; secondly, to gather these up and form a real image within the tube of the microscope. Amici had accomplished this by a special combination of lenses which bears his name; it might, however, be done simply by placing a screwed diaphragm on the end of the upper draw-tube within the body of the microscope. The screw should be that ordinarily used for object-glasses. To this an object-glass of long focus was fitted, and another of higher magnifying-power in the usual place. The whole body was then drawn out and adjusted to a telescopic focus on a distant object. The lower objective formed the object-glass of the telescope, and the inner objective with the Huygenian eyepiece a compound ocular. On reinserting the body thus arranged, and illuminating the crystal on the stage with convergent light by means of a condenser, the rings and brushes could be perfectly seen. The whole double series of rings in a biaxial crystal of carbonate of lead was thus shown.

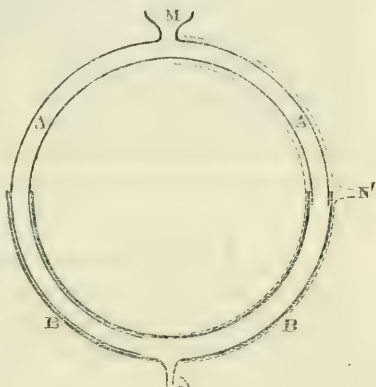
The condenser used was a "kettle-drum" of two plano-convex lenses. The objective on the nozzle of the microscope was a $\frac{2}{3}$ of Ross; that within the draw-tube a 3-inch objective of the same maker.

* Read before the Physical Society, June 13, 1874. Communicated by the Society.

XXII. *Modification of the usual Trombone Apparatus for showing the Interference of Sound-bearing Waves.* By W. F. BARRETT, F.R.S.E. &c., Professor of Physics in the Royal College of Science, Dublin*.

A SIMPLE apparatus for showing the interference of sound-bearing waves may be made by employing a circular arrangement of tubes, one sliding within the other. One tube, A, to which the mouthpiece M is fixed, is three fourths of a circle; the other tube, B, to which the nozzle N is attached, is half a circle, and of such diameter that it slides freely over the tube A.

When the nozzle is diametrically opposite the mouthpiece, the path of the sound-waves is of equal length, and hence the sound from any convenient source placed near to or within the mouthpiece is distinctly heard. By turning the nozzle towards N', in the direction shown by the dotted lines, one limb of the tube is lengthened whilst the other is correspondingly shortened; the path of the waves being now unequal, a point is soon reached where the sound is nearly obliterated.



Employing a suitable source of sound, and a sensitive flame or a resonant jar as a phonoscope, an audience can perceive at once the gradual destruction of the sonorous pulses; and moreover the relative lengths of the two branches of the tube clearly indicate the principle of interference thus illustrated.

One instrument I made was 2 feet in diameter, of 1-inch-square zinc tubing; another and better instrument (skilfully made by Mr. R. H. Ridout) was of brass tubing, 1 foot in diameter, the one limb being $\frac{1}{2}$ -inch, the other $\frac{5}{8}$ -inch tube. About 18 inches in diameter would probably be the best and most convenient size. In making the experiment, care should be taken to avoid (α) the conduction of sound to the ear by the metal substance of the instrument; (β) the direct transmission of sound through the surrounding air. The latter can be overcome by attaching a sufficiently long gutta-percha tube to M,

* Read before the Physical Society, June 20, 1874. Communicated by the Society.

thus removing the mouthpiece to a distance from the ear. The former can be obviated to some extent by having an inelastic mouthpiece or similar covering to the end of the tube. But Mr. Woodward's device of putting a source of sound, such as a reed, entirely within the tube, and a trumpet mouthpiece at N, is undoubtedly the best and most suitable class method of making the experiment.

P.S.—With an ordinary pitch-pipe inserted at N, I have to-day (July 25) repeated the experiment to the class of science teachers now at South Kensington. A continuous blast of air was driven through the pipe from an acoustic bellows; and the loud note heard at first was *utterly extinguished* by altering the relative lengths of the tubes. By pushing the tube still further round the note again came out; thus the sound of the pitch-pipe could be turned on and off at pleasure. Extinction is not confined to a mere line in adjusting the pipe, but spreads over a short and definite range. In this case it is probably, as Professor Goodeve suggests, the interference of two resonant columns of air, rather than the coalescence of two progressive waves in opposite phases.

XXIII. Notices respecting New Books.

Statique Expérimentale et Théorique des Liquides soumis aux seules Forces Moléculaires. Par J. PLATEAU. 2 vols. 8vo, pp. 450 & 495. Ghent and Leipzig: F. Clemm. London: Trübner & Co. 1873.

THIS work consists essentially of the collected series of papers "On the Figures of Equilibrium of a Liquid Mass without Weight," which the distinguished physicist of Ghent has published in the 'Memoirs of the Belgian Academy of Sciences' during the years 1843 to 1868. The substance of these papers having appeared from time to time in the pages of the 'Philosophical Magazine,' in the form of comparatively full abstracts of the original memoirs, it is not needful to say much here by way of introducing or recommending the work to our readers. It should be observed, however, that this book is not merely a republication, offering simply the convenience of presenting in a collected form results which were previously accessible only in a number of separate papers published at intervals during a period of twenty-five years; thanks to the careful revision which the whole has received, and to numerous additions (some of them of considerable extent, relating chiefly to the work of other investigators in the same field of research), the work before us possesses much of the continuity and completeness of a systematic treatise.

The chief scientific interest of the phenomena which Professor Plateau has investigated lies in the simplicity of the physical principle to which they are all of them referrible, and in the compre-

hensiveness of the geometrical relation which forms the mathematical expression of this principle. But, independently of these characters, which are inherent in the nature of the phenomena, and not liable to modification in consequence of the greater or less power brought to the study of them, the present book derives a special value and beauty from the sagacity with which the author has followed out the physical and mathematical consequences involved in the principle of the equality in all directions of the tension of a liquid surface, and in the resulting geometrical relation of the constancy of the sum of the principal curvatures of such a surface, combined with the completeness and accuracy of the experimental verification of theoretical deductions which he has obtained. In fact, the judgment and ingenuity shown in devising the methods of experiment, and the skill with which they have been applied, have enabled the author to trace out, with a minuteness that has not often been equalled in other branches of Physics, the characteristics of the phenomena under investigation. These phenomena also being comparatively simple, in the sense of its being possible to isolate almost completely by the methods adopted the effects of the particular causes it was the author's object to study, these researches form a remarkable example of the close correspondence between theory and experiment, worthy to be compared with Schwerd's memorable work on the Phenomena of Diffraction, a work with which Professor Plateau's presents another point of analogy in the familiar, every-day character of many of the phenomena with which it deals.

Contributions to Selenography. By WILLIAM RADCLIFF BIRT, F.R.A.S., F.M.S. London: Taylor and Francis. 1874.

We are glad to see, by a copy of the above work which we have received for review, that Mr. Birt has put together in one volume his more recent labours connected with Selenography; for not only are there to be found among them able discussions of matters connected very closely with interesting questions of present interest in the science, but we are convinced, from a careful examination of Mr. Birt's production, that it will prove of great value to every student of the lunar surface who may possess a copy—and that not only because in future years it will be a work to which the amateur may turn to compare his own observations with those there recorded of some of the most minute of all lunar objects, in the full confidence that they were carefully drawn and correctly described for the epochs of observation, but because it is a volume likely to be of essential service to every real student in connexion with his own method and mode. Readers of the Reports of the British Association for the Advancement of Science will remember that in 1864 the Association voted a grant for the purpose of mapping the surface of the moon, which was continued for three years—the result being that three areas of the contemplated map, on a scale of 200 inches to the moon's diameter, by Mr. Birt, with catalogues of the objects, were published in the volumes for 1866 and 1868. The first of Mr. Birt's contributions to Selenography, published inde-

pendently of the Association in 1870, is a fourth area of the map, in continuation of the original plan, and which occupies the first place in the present volume. Facing page 1 we have an excellent map of the area, carefully drawn in outline, accompanied by a full descriptive catalogue of 99 craters and other objects situated upon the area. The description is completed by a comparison of four photograms. The numerous notes and woodcuts of interesting objects must be highly suggestive to every earnest student.

The very complete monograph of the *Mare Serenitatis* is of itself a work capable of sustaining the reputation of the author of the four areas, comprising as it does so large a descriptive catalogue of objects within this large and perhaps best-known of all lunar plains, supplemented by copious notes, and illustrated by a map completely crowded with objects, some of them very small indeed. As far as we are able to judge, it is quite a model production. It also contains a very interesting examination of Schröter's drawings of the region, and a comparison of them with recent photograms and the present appearance of the plain.

Hipparchus is the subject of another masterly monograph, illustrated by a well-engraved map, accompanied by a full catalogue of objects and numerous descriptive notes, together with a comparison of the region on different photograms. The scale of the map is 100 inches to the moon's diameter. We notice that the paging of the letterpress of *Hipparchus* runs on from that of the *Mare Serenitatis*, from which we suppose other monographs are to follow. Certainly every lunar observer must hope that may be the case; indeed the continuance of the areas of the map is a very desirable thing while we have nothing at all of the kind which depicts one hundredth of the lunar features revealed by the average telescope now in the hands of amateurs. Beer and Mädler's map was a worthy work in 1837; but nearly forty years have brought about great improvements in instruments for the purpose of observation, and, as it seems to us, a map which would bring selenography more nearly level with the times is really an important desideratum.

Following the three maps to which we have referred, we have specimens of the Catalogue of Lunar Objects according to the plan originally devised by Mr. Birt. This catalogue certainly has the merit of clearness and conciseness; and, by means of a most useful accompanying table of references and synonyms, the student is able easily to compare the notes of different observers and authors on each particular locality which may be under discussion. This is a valuable adjunct to the descriptive notes and illustrations. What our star-catalogues are to stellar observers, that would Mr. Birt's projected work be to students of the moon, if it were only carried out to completion. The method of arrangement adopted throughout all Mr. Birt's productions seems to be a *spécialité* of his own. Other works on the moon we could name, written in what is called the popular style, and illustrated by excellent pictorial representations of the *general* character of the lunar surface; but from all

these, which are more suited to the general reader, the volume before us differs in kind; and those who desire to be really acquainted with *the minuter details* of the various regions treated of will find that Mr. Birt's work treats of these especially. Herein it is unique, and contains a mass of valuable information to be met with, so far as we know, in no other work extant. Indeed all Mr. Birt's maps and notes are distinguished by a painstaking accuracy that will confer upon them great value should another case arise similar to that of *Linné* in any of the areas already completed; for there will be found every known spot, streak, craterlet, or other feature described, and often distinctly illustrated; so that, so far as this work is concerned, no future selenographer will be likely to be misled.

Another portion of the volume is occupied by two series of papers, entitled "Selections from the Portfolios of the Editor of the Lunar Map and Catalogue," in the preparation of which Mr. Birt has been assisted by gentlemen who have given some attention to selenography, and in which will be found many very interesting papers. Especially noticeable is one by the Rev. T. W. Webb, "On the Study of Change in the Lunar Surface," and another by Messrs. Webb and Birt on the formation named *Cleomedes*. The latter contains formulæ for computing the length of a measured line on the moon's surface in English feet, in itself a really important acquisition to every selenographer. Many other papers, treating of various topics, will be found suggestive.

From a notice on the wrapper of the second issue of the "Selections," we learn that increased subscriptions are required to continue them. But we cannot suppose that the want of subscriptions is dependent upon any inferiority in the work itself, but rather on its being not generally known amongst astronomers, and also on the absence of an interest in the study of the moon's surface, which contrasts so remarkably with the assiduity with which amateurs prosecute their studies in other branches of astronomy. We therefore hope that before long we shall be called upon to notice a further contribution to selenography by Mr. Birt.

XXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 72.]

January 29, 1874.—Joseph Dalton Hooker, C.B., President, in the Chair.

THE following communication was read:—

"On the Comparative Value of certain Geological Ages (or groups of formations) considered as items of Geological Time."
By A. C. Ramsay, LL.D., V.P.R.S.

The author first reviews briefly several methods by which attempts have been made to estimate the value of minor portions

of geological time, such as:—calculations intended to estimate the age of deltas, founded on the annual rate of accumulation of sediments; the astronomical method followed by Mr. Croll, in connexion with the recurrence of glacial epochs; the relative thicknesses of different formations; and the relation of strong unconformity between two sets of formations in connexion with marked disappearance of old genera and species, and the appearance of newer forms. Having shown that none of these methods give any clear help in the absolute measurement of time in years or cycles of years, even when founded on well-established facts, he proceeds to attempt to estimate the *comparative value* of long portions of geological time, all of which are represented by important series of formations.

The author then alludes to the subject of two papers by himself, given to the Geological Society in 1871, on the Red Rocks of England, in which he attempted to show that the Old Red Sandstone, Permian, and New Red series were all deposited in great inland lakes, fresh or salt; and this, taken in connexion with the wide-spreading terrestrial character of much of the Carboniferous series, showed that a great continental age prevailed over much of Europe and in some other regions, from the close of the Silurian epoch to the close of the Trias. He then endeavours to show the *value* of the time occupied in the deposition of the above-named formations, when compared with the time occupied in the deposition of the Cambrian and Silurian strata, and of the marine and freshwater strata which were deposited between the close of the Triassic epoch and the present day.

After alluding to the probable mixed estuarine and marine character of the purple and grey Cambrian rocks of St. David's, it is shown that the Cambrian and Silurian series may be massed into three great groups:—first, from the bottom of the purple Cambrian rocks to the top of the Tremadoc slates; these being succeeded unconformably by the second group, the Llandeilo and Bala or Caradoc beds; on which rest unconformably the members of the third series, ranging from the base of the Upper Llandovery to the top of the Upper Ludlow beds,—each unconformable break in stratigraphical succession being accompanied by a corresponding break in palæontological succession.

These three great divisions are next shown to be comparable, in the time occupied for their deposition, to the three divisions of Lower, Middle, and Upper Devonian rocks, which are considered to be the marine representatives of the Old Red Sandstone; and therefore it follows that *the time occupied in the deposition of the latter may have been as long as that taken in the deposition of the Cambrian and Silurian series.* This position is strengthened by the great palæontological differences in the fossils of the Upper Ludlow and those of the marine Carboniferous series, which seem to indicate a long lapse of time during which, in Old Red Sandstone areas, no direct sequence of marine deposits took place.

The next question considered is, what relation in point of time

the deposition of the Old Red Sandstone may have taken, when compared with the time occupied in the deposition of certain members of the Mesozoic formations. Through a series of arguments, lithological, stratigraphical, and palæontological, the conclusion is arrived at, that the whole of the Liassic and Oolitic series present the various phases of one facies of marine life, and, in this respect, are comparable to the changes in the fossil contents of the various subformations of the Cambrian and Lingula-flag series, of which the Tremadoc Slates form an upper member. In like manner the Lias and Oolites may be compared with the Lower Devonian strata; and therefore *a lower portion of the Old Red Sandstone may have taken as long for its deposition as the whole of the time occupied in the deposition of the Jurassic series.*

Following out this train of argument through the Neocomian and Cretaceous strata, the result is arrived at *that the whole of the time occupied in the deposition of the Old Red Sandstone may have been equal to the whole of the time occupied in the deposition of all the Jurassic, Wealden, and Cretaceous strata collectively.*

In the same manner the next term of the Continental era, the Carboniferous epoch, is compared with the Eocene period, both being locally of marine, estuarine, freshwater, and terrestrial origin, and both connected with special continental epochs. Next comes the Permian series, comparable in its lacustrine origin to the Miocene strata of so much of Europe, though in the case of the Permian waters the lakes were salt. After this the Triassic series of Europe alone remains of the old continent, the marine and salt-lake strata of which are not likely to have taken a shorter time in their deposition than the older Pliocene strata.

If the foregoing method be of value, we arrive at the general conclusion that *the great local continental era, which began with the Old Red Sandstone and closed with the New Red Marl, is comparable, in point of Geological Time, to that occupied in the deposition of the whole of the Mesozoic series later than the New Red Marl, and of all the Cainozoic formations, and, more probably, of all the time that has elapsed since the beginning of the deposition of the Lias down to the present day;* and consequently the more modern continental era, which locally began with the Eocene period and lasts to the present day, has been of much shorter duration.

The author then pointed out that during the older continental era there flourished two typical floras—one extending from the time of the Old Red Sandstone to the close of the Permian strata; while the second, which is largely of Jurassic type, characterized the Triassic formations. From the time of the Lias onward in time, we have also two distinct typical floras—the first of Jurassic, and the second of much more modern type, beginning with the Upper Cretaceous strata of Aix-la-Chapelle and lasting to the present day.

In like manner the faunas connected with the land resolve themselves into two types:—the first chiefly Labyrinthodontian, as shown in the Carboniferous and Permian strata; and the second charac-

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teristic of the Trias, with 'Crocodilia, many land-lizards, Anomodontia, Deinosauria, and Marsupial Mammalia. This fauna, as regards genera, with the exception of Labyrinthodontia and the appearance of Pterosauria, is represented through the remaining members of the Mesozoic formations, from Jurassic to Cretaceous inclusive. After this comes the Pachydermatous Mammalian Eocene fauna, and after that the Miocene land-fauna, which, in its main characters, is of modern type. From Jurassic to Cretaceous times, inclusively, there was therefore, as far as we know, in this area a land-fauna chiefly Reptilian, Saurian, and Marsupial, and in Tertiary times chiefly Reptilian and Placental. (Illustrated by a Table.)

In conclusion, the recent character of the early marine faunas of the Cambrian and Lingula-flag series was pointed out, such as Spongida, Annelida, Echinodermata, Crustacea, Polyzoa, Brachiopoda, Lamellibranchiata, Pteropoda, Nucleobranchiata, and Cephalopoda. This was long ago insisted on by Professor Huxley; and we find no evidence of its having lived near the beginning of the zoological series; for below the Cambrian series we are at once involved in a sort of chaos of metamorphic strata. Of the geological history, in the words of Darwin, "we possess the last volume alone, relating only to two or three countries." The connexion of this question with that of the comparative value of different geological eras is obvious, especially in relation to the palæontological part of the question.

June 18.—Joseph Dalton Hooker, C.B., President, in the Chair.

The following communication was read :—

"On the Forces caused by Evaporation from, and Condensation at, a Surface." By Prof. Osborne Reynolds, of Owens College, Manchester.

It has been noticed by several philosophers, and particularly by Mr. Crookes, that, under certain circumstances, hot bodies appear to repel and cold ones to attract other bodies. It is my object in this paper to point out, and to describe experiments to prove, that these effects are the results of evaporation and condensation, and that they are valuable evidence of the truth of the kinetic theory of gas, viz. that gas consists of separate molecules moving at great velocities.

The experiments of which the explanation will be given were as follows :—

A light stem of glass, with pith-balls on its ends, was suspended by a silk thread in a glass flask, so that the balls were nearly at the same level. Some water was then put in the flask and boiled until all the air was driven out of the flask, which was then corked and allowed to cool. When cold there was a partial vacuum in it, the gauge showing from $\frac{1}{2}$ to $\frac{3}{4}$ of an inch pressure.

It was now found that when the flame of a lamp was brought near to the flask, the pith-ball which was nearest the flame was driven away, and that with a piece of ice the pith was attracted.

This experiment was repeated under a variety of circumstances, in different flasks and with different balances, the stem being sometimes of glass and sometimes of platinum; the results, however, were the same in all cases, except such variations as I am about to describe.

The pith-balls were more sensitive to the heat and cold when the flask was cold and the tension within it low; but the effect was perceptible until the gauge showed about an inch, and even after that the ice would attract the ball.

The reason why the repulsion from heat was not apparent at greater tensions, was clearly due to the convection-currents which the heat generated within the flask. When there was enough vapour, these currents carried the pith with them; they were, in fact, then sufficient to overcome the forces which otherwise moved the pith. This was shown by the fact that when the bar was not quite level, so that one ball was higher than the other, the currents affected them in different degrees; also that a different effect could be produced by raising or lowering the position of the flame.

The condition of the pith also perceptibly affected the sensitiveness of the balls. When a piece of ice was placed against the side of the glass, the nearest of the pith-balls would be drawn towards the ice, and would eventually stop opposite to it. If allowed to remain in this condition for some time, the vapour would condense on the ball near the ice, while the other ball would become dry (this would be seen to be the case, and was also shown by the tipping of the balance, that ball against the ice gradually getting lower). It was then found, when the ice was removed, that the dry ball was insensible to the heat, or nearly so, while that ball which had been opposite to the ice was more than ordinarily sensitive.

If the flask were dry and the tension of the vapour reduced with the pump until the gauge showed $\frac{2}{3}$ of an inch, then, although purely steam, the vapour was not in a saturated condition, and the pith-balls which were dry were no longer sensitive to the lamp, although they would still approach the ice.

From these last two facts it appears as though a certain amount of moisture on the balls were necessary to render them sensitive to the heat.

In order that these results might be obtained, it was necessary that the vapour should be free from air. If a small quantity of air was present, although not enough to appear in the gauge, the effects rapidly diminished, particularly that of the ice, until the convection-currents had it all their own way. This agrees with the fact that the presence of a small quantity of air in steam greatly retards condensation and even evaporation.

With a dry flask and an air-vacuum, neither the lamp nor the ice produced their effects; the convection-currents reigned supreme even when the gauge was as low as $\frac{1}{4}$ inch. Under these circumstances the lamp generally attracted the balls and the ice repelled

them; *i. e.* the currents carried them towards the lamp and from the ice; but, by placing the lamp or ice very low, the reverse effects could be obtained, which goes to prove that they were the effects of the currents of air.

These experiments appear to show that evaporation from a surface is attended with a force tending to drive the surface back, and condensation with a force tending to draw the surface forward. These effects admit of explanation, although not quite as simply as may at first sight appear.

It seems easy to conceive that when vapour is driven off from a body there must be a certain reaction or recoil on the part of the body; Hero's engine acts on this principle. If a sheet of damp paper be held before the fire, from that side which is opposite to the fire a stream of vapour will be drawn off towards the fire with a perceptible velocity; and therefore we can readily conceive that there must be a corresponding reaction, and that the paper will be forced back with a force equal to that which urges the vapour forwards. And, in a similar way, whenever condensation goes on at a surface it must diminish the pressure at the surface, and thus draw the surface forwards.

It is not, however, wholly, or even chiefly, such visible motions as these that afford an explanation of the phenomena just described. If the only forces were those which result from the perceptible motion, they would be insensible, except when the heat on the surface was sufficiently intense to drive the vapour off with considerable velocity. This, indeed, might be the case if vapour had no particles and was, what it appears to be, a homogeneous elastic medium, and if, in changing from liquid into gas, the expansion took place gradually, so that the only velocity acquired by the vapour was that necessary to allow its replacing that which it forces before it and giving place to that which follows.

But, although it appears to have escaped notice so far, it follows, as a direct consequence of the *kinetic* theory of gases, that, whenever evaporation takes place from the surface of a solid body or a liquid, it must be attended with a reactionary force equivalent to an increase of pressure on the surface, which force is quite independent of the perceptible motion of the vapour. Also, condensation must be attended with a force equivalent to a diminution of the gaseous pressure over the condensing surface, and likewise independent of the visible motion of the vapour. This may be shown to be the case as follows :—

According to the kinetic theory, the molecules which constitute the gas are in rapid motion, and the pressure which the gas exerts against the bounding surfaces is due to the successive impulses of these molecules, whose course directs them against the surface, from which they rebound with unimpaired velocity. According to this theory, therefore, whenever a molecule of liquid leaves the surface henceforth to become a molecule of gas, it must leave it with a velocity equal to that with which the other particles of gas rebound; that is to say, instead of being just detached and quietly

passing off into the gas, it must be shot off with a velocity greater than that of a cannon-ball. Whatever may be the nature of the forces which give it the velocity, and which consume the latent heat in doing so, it is certain, from the principle of conservation of momentum, that they must react on the surface with a force equal to that exerted on the molecule, just as in a gun the pressure of the powder on the breech is the same as on the shot.

The impulse on the surface from each molecule which is driven off by evaporation must therefore be equal to that caused by the rebound of one of the reflected molecules, supposing all the molecules to be of the same size; that is to say, since the force of rebound will be equal to that of stopping, the impulse from a particle driven off by evaporation will be half the impulse received from the stopping and reflection of a particle of the gas. Thus the effect of evaporation will be to increase the number of impulses on the surface; and although each of the new impulses will only be half as effective as the ordinary ones, they will add to the pressure.

In the same way, whenever a molecule of gas comes up to a surface and, instead of rebounding, is caught and retained by the surface, and is thus condensed into a molecule of liquid, the impulse which it will thus impart to the surface will only be one half as great as if it had rebounded. Hence condensation will reduce the magnitude of some of the impulses, and therefore will reduce the pressure on the condensing surface.

For instance, if there were two surfaces in the same vapour, one of which was dry and the other evaporating, then the pressure would be greater on the moist surface than on that which was dry. And, again, if one of the surfaces were dry and the other condensing, then the pressure would be greater on the dry surface than on that which was condensing. Hence, if the opposite sides of a pith-ball in vapour were in such different conditions, the ball would be forced towards the colder side.

These effects may be expressed more definitely as follows:—

Let v be the velocity with which the molecules of the vapour move,

p the pressure on a unit of surface,

d the weight of a unit of volume of the vapour,

w the weight of liquid evaporated or condensed in a second;

then the weight of vapour which actually strikes the unit of dry surface in a second will be

$$= \frac{dv}{6},$$

and the pressure p will be given by

$$p = 2 \frac{dv^2}{6g}^*,$$

and f (the force arising from evaporation) will be given by

$$f = \frac{wv}{g};$$

* See Maxwell, 'Theory of Heat,' p. 294.

therefore

$$f = w \sqrt{\frac{3p}{gd}}$$

Thus we have an expression for the force in terms of the quantity of water evaporated and the ratio of the pressure to the density of the vapour; and if the heat necessary to evaporate the liquid (the latent heat) is known, we can find the force which would result from a given expenditure of heat.

Applying these results to steam, we find that, at a temperature of 60°, the evaporation of 1 lb. of water from a surface would be sufficient to maintain a force of 65 lbs. for one second.

It is also important to notice that this force will be proportional to the square root of the absolute temperature, and, consequently, will be approximately constant between temperatures of 32° and 212°.

If we take mercury instead of water, we find that the force is only 6 lbs. instead of 65 lbs.; but the latent heat of mercury is only $\frac{1}{30}$ that of water, so that the same expenditure of heat would maintain nearly three times as great a force.

It seems, therefore, that in this way we can give a satisfactory explanation of the experiments previously described. When the radiated heat from the lamp falls on the pith, its temperature will rise, and any moisture on it will begin to evaporate and to drive the pith from the lamp. The evaporation will be greatest on that ball which is nearest to the lamp; therefore this ball will be driven away until the force on the other becomes equal, after which the balls will come to rest, unless momentum carries them further. On the other hand, when a piece of ice is brought near, the temperature of the pith will be reduced, and it will condense the vapour and be drawn towards the ice.

It seems to me that the same explanation may be given of Mr. Crookes's experiments; for, although my experiments were made on water and at comparatively high pressures, they were in reality undertaken to verify the explanation as I have given it. I used water in the hope of finding (as I have found) that, in a condensable vapour, the results could be obtained with a greater density of vapour (that is to say, with a much less perfect vacuum), the effect being a consequence of the saturated condition of the vapour rather than of the perfection of the vacuum.

Mr. Crookes only obtained his results when his vacuum was nearly as perfect as the Sprengel pump would make it. Up to this point he had nothing but the inverse effects, viz. attraction with heat and repulsion with cold. About the cause of these he seems to be doubtful; but I venture to think that they may be entirely explained by the expansion of the surrounding gas or vapour, and the consequent convection-currents. It must be remembered that whenever the air about a ball is expanded, and thus rendered lighter by heat, it will exercise less supporting or floating power on the ball, which will therefore tend to sink. This tendency will

be in opposition to the lifting of the ascending current, and it will depend on the shape and thickness of the ball whether it will rise or fall when in an ascending current of heated gas.

The reason why Mr. Crookes did not obtain the same results with a less perfect vacuum was because he had then too large a proportion of air, or non-condensing gas, mixed with the vapour, which also was not in a state of saturation. In his experiments the condensable vapour was that of mercury, or something which required a still higher temperature, and it was necessary that the vacuum should be very perfect for such vapour to be any thing like pure and in a saturated condition. As soon, however, as this state of perfection was reached, then the effects were more apparent than in the corresponding case of water. This agrees well with the explanation; for, as previously shown, the effect of mercury would, for the same quantity of heat, be three times as great as that of water; and, besides this, the perfect state of the vacuum would allow the pith (or whatever the ball might be) to move much more freely than when in the vapour of water at a considerable tension.

Of course this reasoning is not confined to mercury and water; any gas which is condensed or absorbed by the balls when cold in greater quantities than when warm would give the same results; and, as this property appears to belong to all gases, it is only a question of bringing the vacuum to the right degree of tension.

There was one fact connected with Mr. Crookes's experiments which, independently of the previous considerations, led me to the conclusion that the result was due to the heating of the pith, and was not a direct result of the radiated heat.

In one of the experiments exhibited at the *Soirée* of the Royal Society, a candle was placed close to a flask containing a bar of pith suspended from the middle: at first, the only thing to notice was that the pith was oscillating considerably under the action of the candle; each end of the bar alternately approached and receded, showing that the candle exercised an influence similar to that which might have been exercised by the torsion of the thread had this been stiff. After a few minutes' observation, however, it became evident that the oscillations, instead of gradually diminishing, as one naturally expected them to do, continued; and, more than this, they actually increased, until one end of the bar passed the light, after which it seemed quieter for a little, though the oscillations again increased until it again passed the light. As a great many people and lights were moving about, it seemed possible that this might be due to external disturbance, and so its full importance did not strike me. Afterwards, however, I saw that it was only to be explained on the ground of the force being connected with the temperature of the pith. During part of its swing one end of the pith must be increasing in temperature, and during the other part it must be cooling. And it is easily seen that the ends will not be hottest when nearest the light, or coldest when furthest away; they

will acquire heat for some time after they have begun to recede, and lose it after they have begun to approach. There will, in fact, be a certain lagging in the effect of the heat on the pith, like that which is apparent in the action of the sun on a comet, which causes the comet to be grandest after it has passed its perihelion. From this cause it is easy to see that the mean temperature of the ends will be greater during the time they are retiring than while approaching, and hence the driving force on that end which is leaving will, on the whole, more than balance the retarding force on that which is approaching; and the result will be an acceleration, so that the bar will swing further each time until it passes the candle, after which the hot side of the bar will be opposite to the light, and will for a time tend to counteract its effect, so that the bar will for a time be quieter. This fact is independent evidence as to the nature of the force; and although it does not show it to be evaporation, it shows that it is a force depending on the temperature of the pith, and that it is not a direct result of radiation from the candle.

Since writing the above paper, it has occurred to me that, according to the kinetic theory, a somewhat similar effect to that of evaporation must result whenever heat is communicated from a hot surface to gas.

The particles which impinge on the surface will rebound with a greater velocity than that with which they approached; and consequently the effect of the blow must be greater than it would have been had the surface been of the same temperature as the gas.

And, in the same way, whenever heat is communicated from a gas to a surface, the force on the surface will be less than it otherwise would be, for the particles will rebound with a less velocity than that at which they approach.

Mathematically the result may be expressed as follows—the symbols having the same meaning as before, ϵ representing the energy communicated in the form of heat, and δv the alteration which the velocity of the molecule undergoes on impact. As before,

$$p = \frac{dv^2}{3g} \quad \text{or} \quad v = \sqrt{\frac{3gp}{d}};$$

and

$$\epsilon = \frac{dv}{6} \frac{(v + \delta v)^2 - v^2}{2g} = \frac{dv^2 \delta v}{6g} \quad \text{nearly,}$$

$$f = \frac{dv}{6g} \delta v;$$

$$\therefore f = \frac{\epsilon}{p} = \epsilon \sqrt{\frac{d}{3gp}}.$$

Therefore, in the case of steam at a temperature of 60° ,

$$f = \frac{\epsilon}{2000};$$

and in the case of air

$$f = \frac{\epsilon}{1400}.$$

It must be remembered that ϵ depends on the rate at which cold particles will come up to the hot surface, which is very slow when it depends only on the diffusion of the particles of the gas *inter se* and the diffusion of the heat amongst them.

It will be much increased by convection-currents; but these will (as has been already explained), to a certain extent, produce an opposite effect. It would also seem that this action cannot have had much to do with Mr. Crookes's experiments, as one can hardly conceive that much heat could be communicated to the gas or vapour in such a perfect vacuum as that he obtained, unless, indeed, the rate of diffusion varies inversely as the density of a gas*. It will be interesting, however, to see what light experiments will throw on the question.

GEOLOGICAL SOCIETY.

[Continued from p. 76.]

November 5, 1873.—Prof. Ramsay, F.R.S., Vice-President,
in the Chair.

The following communications were read:—

1. "On the Skull of a Species of *Halitherium* from the Red Crag of Suffolk." By Prof. W. H. Flower, F.R.S., F.G.S.

The specimen described, which is in the collection of the Rev. H. Canham, of Waldringfield, is from the so-called coprolite- or bone-bed at the base of the Red Crag, and presents the usual aspect of the mammalian remains from that bed. It is of especial interest as furnishing the first recorded evidence of the existence in Britain of animals belonging to the order Sirenia. The fragment consists of the facial part of the cranium, separated, probably before fossilization, from the posterior part at the fronto-parietal suture, and in a line descending vertically therefrom. It was afterwards subjected to severe attrition, by which many of the projecting parts have been removed; but sufficient remains to enable its general relationship to known forms to be determined. The whole of that portion of the maxillæ in which the molar teeth were implanted is preserved.

The author compared the fossil skull with those of the existing and extinct species of the order, and stated that, while it presents many characters common to the Manati and the Dugong, there are others by which it differs from both, the most striking being the more normal development of the nasal bones and the outer wall of the nasal fossæ, and especially the dentition, in all of which it shows a more generalized condition. The existence in it of maxillary teeth removes it still further from *Rhytina*. In general character the

* June 10.—Professor Maxwell has shown that the diffusion both of heat and of the gas varies inversely as the density; therefore, excepting for convection-currents, the amount of heat communicated from a surface to a gas would be independent of the density of the gas, and hence the force f would be independent of the density; that is to say, this force would remain constant as the vacuum improved, while the convection-currents and counteracting forces would gradually diminish. It seems probable, therefore, that Mr. Crookes's results are, at least in part, due to this force.

molars correspond with those of the genus *Halitherium*, in which the author considered that this fossil found its nearest ally in *H. Schinzi*, Kaup, from the Miocene of the Rhine Valley, a formation in which several of the animals of the Red-Crag bone-bed are known to occur. The differences, however, especially the larger size of the cranium, in the Crag specimen, and the larger size of its teeth, induce the author to regard it as a distinct species, which he proposes to name *Halitherium Canhami*.

2. "New Facts bearing on the Inquiry concerning Forms intermediate between Birds and Reptiles." By Henry Woodward, Esq., F.R.S., F.G.S.

The author, after giving a brief sketch of the Sauropsida, and referring especially to those points in which the Pterosaurians approach and differ from birds, spoke of the fossil birds and land reptiles which he considered to link together more closely the Sauropsida as a class.

The most remarkable recent discoveries of fossil birds are :—

I. *Archæopteryx macrura* (Owen), a Mesozoic type, which has a peculiar reptilian-like tail, composed of twenty free and apparently unanchylosed cylindrical vertebræ, each supporting a pair of quill-feathers, the last fifteen vertebræ having no transverse processes, and tapering gradually to the end.

II. *Ichthyornis dispar* (Marsh), discovered by Prof. O. C. Marsh in 1872 in the Upper Cretaceous beds of Kansas, U. S. It possessed well-developed teeth in both jaws. The teeth are set in distinct sockets, and are all more or less inclined backwards.

III. *Odontopteryx toliapica* (Owen), an Eocene bird from the London Clay of Sheppey, the skull of which alone has been discovered, has very prominent denticulations of the alveolar margins of the jaws.

The author then referred to the Dinosauria, some of which he considered to present points of structure tending towards the so-called wingless birds.

I. *Compsognathus longipes* (A. Wagner), from the Oolite of Solenhofen, is about two feet in length, having a small head with toothed jaws, supported on a long and slender neck.

The iliac bones are prolonged in front of and behind the acetabulum; the pubes are long and slender. The bones of the fore limbs are small, and were probably furnished with two clawed digits. The hind limb is very large, and disposed as in birds, the femur being shorter than the tibia. The proximal division of the tarsus is anchylosed with the tibia as in birds.

II. The huge carnivorous *Megalosaurus*, ranging from the Lias to the Wealden, had strong but not massive hind limbs, and short reduced fore limbs; it moved with free steps, chiefly if not solely on its hind limbs, which is true also of the vegetable-eating lizards of the Mesozoic rocks.

The author next drew attention to the Frilled Lizard of Australia, *Chlamydosaurus Kingii* (Gray), which has its fore limbs very much smaller than the hind limbs, and has been observed not only to sit

up occasionally, but to run habitually upon the ground on its hind legs, its fore paws not touching the earth, which upright carriage necessitates special modifications of the sacrum and pelvic bones.

The Solenhofen Limestone, in which Pterosauria are frequent, and which has yielded the remains of *Archæopteryx* and of *Compsognathus*, has also furnished a slab bearing a bipedal track, resembling what might be produced by *Chlamydosaurus* or *Compsognathus*. It shows a median track formed by the tail in being drawn along the ground; on each side of this the hind feet with outspread toes leave their mark, while the fore feet just touch the ground, leaving dot-like impressions nearer the median line. Hence the author thought that, while some of the bipedal tracks which are met with from the Trias upwards may be the "spoor" of struthious birds, most of them are due to the bipedal progression of the Secondary Reptiles.

3. "Note on the Astragalus of *Iguanodon Mantelli*." By J. W. Hulke, Esq., F.R.S., F.G.S.

The author exhibited and described an astragalus of *Iguanodon* from the collection of E. P. Wilkins, Esq., F.G.S. The bone was believed to be previously unknown. It is a bone of irregular form, having on its lower surface the characteristic pulley-shape of a movable hinge-joint. The upper surface presents a form exactly adapted to that of the distal end of the tibia; so that the applied surfaces of the astragalus and tibia must have interlocked in such a manner as to have precluded all motion between them. The author remarked upon the interest attaching to this fact in connexion with the question of the relationship between the Dinosauria and Birds.

4. "Note on a very large Saurian Limb-bone, adapted for progression upon land, from the Kimmeridge Clay of Weymouth, Dorset." By J. W. Hulke, Esq., F.R.S., F.G.S.

The bone described by the author presents a closer resemblance to the Crocodilian type of humerus than to any other bone; and he regarded it as the left humerus of the animal to which it belonged. Its present length is 54 inches; but when perfect it could hardly have been less than 63 inches in length. The middle of the shaft is cylindroid. Its transverse section is of a subtrigonal figure, and presents a large coarsely cancellated core, enclosed in a compact cortical ring. The bone is considerably expanded towards the two extremities; the distal articular surface is oblong, and divided into a pair of condyles by a very shallow vertical groove; below, the anterior border, in its proximal half, is much wider than the corresponding portion of the posterior border, and is flattened and produced downwards into a ventrally projecting crest; and the distal half of this border forms a thin, rough crest, projecting forwards. The presence of these crests distinguishes the present humerus from those of *Pelorosaurus* and of *Ceteosaurus oxoniensis*; but the general correspondence of the bone with the humerus of the latter species leads the author to refer it provisionally to a species of *Ceteosaurus*, which he proposes to name *C. humero-cristatus*.

XXV. *Intelligence and Miscellaneous Articles.*

ON A SIMPLE OCULAR-SPECTROSCOPE FOR STARS.

BY F. ZÖLLNER.

THE annexed figure shows, of the natural size, the section of a compendious form of star-spectroscope in combination with the ocular of a telescope.

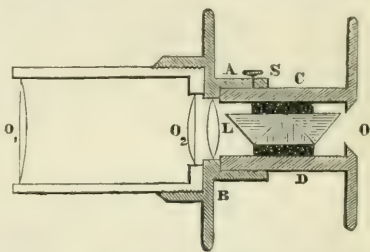
It consists of a small direct-vision prism fixed in a tube C D, the dispersion of which is about equivalent to that of the system of prisms of a Browning miniature spectroscope. The tube C D is movable in a second tube, A B, which can be screwed upon the head of the eyepiece and contains a cylindrical lens L of about 100 millims. focal distance. As the length of the line of light produced by this lens depends both on its focal distance and also on the dimensions and proportions of the optical parts of the telescope, it is advisable to have in readiness several cylindrical lenses of different lengths of focus, so as to be able to employ them according to the length of the line of light (and consequently the breadth of the spectrum) desired.

O₁ and O₂ are the two lenses of the eyepiece, and hence do not belong to the spectroscope.

If with this instrument the spectrum of a star is to be observed, the tube C D with the prism is first removed, and the ocular so arranged that when the eye is at O a sharp line of light is seen. It is essential, in doing this, that the eye should be at about the same distance from the lens L as when the prism is employed. The tube C D is now inserted, in such a manner that the refracting edge of the prism lies, as usual, parallel to the luminous line, and consequently the spectrum attains its greatest breadth. Self-evidently, for a given telescope, the suitable arrangement need only be once ascertained; so that then by a small screw S the prism can be fixed in an invariable position with respect to the cylindrical lens L. The prism is manufactured by M. Merz, of Munich; and he prefers to use it in this compendious form for microscopes.

The intensity of the light of this ocular-spectroscope is so considerable, that, in combination with a small portable telescope, the objective of which has only 35 millimetres aperture and about 400 millims. focal distance, it shows distinctly the lines of stars of the first magnitude, such as Wega, α Orionis, and even α Herculis when the state of the atmosphere corresponds, as Professor Winnecke and Dr. Vogel convinced themselves and others on the occasion of their visit to Leipzig in the course of the past year. When Venus appears as a slender crescent, its spectrum is singularly beautiful.

Although, according to the well-known methods employed by



Browning, Vogel, and others, a scale could be very easily connected with this instrument, it can be recommended even without one for systematic mass-observations of fixed-star spectra, in which the prime object is to ascertain the typical constitution of the spectra. As the essential differences between these types probably depends only on the temperature and mass of those incandescent bodies, and according to the observations of Secchi and others those types stand in a certain relation to the distribution of the stars in space, such systematically conducted mass-observations may in future become of high importance for the progress of astrophysics.

I permit myself, in conclusion, the remark that the combination above described was explained and exhibited by me at the last meeting of the Astronomical Society at Hamburg, in September 1873.—*Berichte der kön. sächs. Gesellschaft der Wissenschaften math.-phys. Classe*, April 23, 1874.

NOTE ON THE CAUSE OF TIDES. BY E. J. CHAPMAN, PH.D., PROFESSOR OF MINERALOGY AND GEOLOGY IN UNIVERSITY COLLEGE, TORONTO*.

The phenomenon of the tides, stated broadly, consists of a passing elevation, real or apparent, of oceanic waters at two opposite points on the surface of the globe. These elevations, which follow the moon in its course, may become greatly intensified under local conditions, as where opposing coast-lines impede the progress of the tidal wave; but in the open ocean, it is well known, they are of but slight significance. According to the received theory, they are occasioned essentially by the unequal degree of attraction exerted by the moon on different parts of the earth—this attraction being, of course, modified by that of the sun. It is thus assumed that the waters, owing to their comparative mobility, are drawn towards the moon on one side of the globe, whilst the solid earth is drawn away from the waters on the other side—or, to use the common phraseology, is drawn towards the moon faster than the waters can follow.

This view, although not without opponents, has been almost universally adopted in default of a more satisfactory explanation.

The explanation of the cause of tides now suggested has at least this merit: it applies the same principle in elucidation of both tides—that nearest the moon, and that on the opposite side of the globe. It is briefly this:—When two bodies pull against each other, there must necessarily be a contraction of particles towards the centre of each body along the line of pull or resistance. In the pull, therefore, of the earth upon the moon, the earth (and of course the moon also) must suffer a passing contraction, the part along the line of pull, so to say, contracting more than the other parts. But this contraction is mechanical only, and is therefore a compression; and

* Communicated by the Author. Condensed from a communication made to the Canadian Institute, February 7, 1874.

as water is practically incompressible, the sea remains essentially unaffected, whilst the earth shrinks beneath it, and thus causes the tide. The shrinkage of course becomes greater, and the tide higher, when both sun and moon take part in the counter-pull, whether acting on the same side of the earth or on opposite sides. It may be assumed, however, from the known height of the tidal wave where the march of this wave is unopposed, that the maximum amount of contraction does not exceed a foot for each thousand miles of the earth's radius—being thus, in round numbers, less than one part in five millions. In the tremendous pull of the earth upon the moon, by which the moon is kept upon its course, a passing contraction of this comparatively slight amount may be easily conceived to follow. According to the commonly adopted theory, one tide is assumed to result from the withdrawal of the earth, locally, from the waters above it: in the view now proposed, both tides are assumed (although on a different principle) to be thus caused.

ON THE TEMPERATURE OF THE SUN. BY J. VIOLLE.

Several months since, I undertook some experiments to determine, by various methods, the temperature of the sun. I beg the Academy to kindly permit me to submit to it the first results of my researches.

Measurements of solar heat can be made in two ways. In the first, a thermometer is placed successively during equal times in the shade and then in the sun, and the course of the instrument is followed in each case: this is the *dynamic method*, that of the pyrohelium of Pouillet. In the second the thermometer remains submitted to solar radiation until the temperature indicated by the instrument becomes stationary; and at the same time the temperature of the thermometer and that of the enclosure are noted: this is the *static method*, that which appears to be adhered to by most of the physicists who occupy themselves with the measurement of solar heat. I shall for the moment speak only of the latter method, and in the first place consider its principle.

Let a spherical envelope be maintained at a constant temperature t , and let the bulb of a thermometer be in the centre of the sphere, which bulb I will for an instant suppose infinitely small. The enclosure is coated with lampblack, as well as the bulb of the thermometer. Let us suppose equilibrium of temperature established. The enclosure then sends to the thermometer a quantity of heat Sat , a being Dulong's constant or 1.0077; and the thermometer sends back to the enclosure the same quantity of heat Sat . Let us now pierce in the spherical enclosure a circular aperture ω of such dimensions that it will be seen from the centre under the angle which measures the apparent diameter of the sun, and let us direct this aperture toward the sun. It is manifest, according to the law of the variation of calorific intensity inversely as the square of the distance, that the real action of the sun on the bulb of the thermo-

meter is identical with that which would be exerted by a disk of surface ω placed at the aperture of our sphere, this disk having the same temperature and emissive power as the sun. We can therefore define the temperature of the sun by that which would have to be attributed to this imaginary disk, possessing the emissive power of lampblack, to produce upon the thermometer the same effect which is actually produced by the sun. Let x be the temperature, thus defined, of the sun, θ the stationary temperature of the thermometer receiving the solar radiation through the aperture ω ; the quantity of heat emitted by the thermometer (which was Sa^t at the temperature t) has become Sa^θ ; and putting that quantity of heat equal to the sum of the quantities emitted by enclosure and by the sun, we have at once

$$Sa^\theta = Sa^t + \omega a^x.$$

This is precisely the equation as written by M. Vicaire; but it was established under reserves from which we must now free ourselves. The dimensions of the thermometer are necessarily finite; and consequently the aperture through which the solar rays penetrate must be widened to permit them to reach the whole of the bulb: hence comes a double complication.

Let us now consider an admission-aperture Ω large enough for an entire hemisphere of the bulb to receive the rays of the sun. If the diameter of the bulb is sufficiently small in proportion to that of the enclosure, every point of it will be sensibly in the same conditions; so that in order to account for the actual state of the apparatus, it is sufficient to consider any one point whatever of the bulb. This point is submitted:—(1) to the radiation of all the preserved portion of the enclosure; (2) to the radiation of the sun, which is equivalent to that of a surface ω placed at a distance equal to the radius of the enclosure and kept at the temperature of the sun; (3) to the radiation of the whole of a portion of the sky bordering the sun, which acts as a surface $\Omega - \omega$ at an unknown temperature y . The precise equation is, therefore,

$$Sa^\theta = Sa^t + \omega a^x + \Omega a^y.$$

I will indicate in a forthcoming note how, making Ω to vary by means of diaphragms pierced with apertures of known dimensions, the correction-term Ωa^y can be determined with sufficient exactness. An idea of its quantity will be given by the following result, the only one I shall cite at present:—

On March 14, 1874, the sky being very clear, although the ground was covered with snow, at 1 p.m. the quantity of heat arriving from the sun at the surface of the ground was the same as that which would have been given by a disk of the same apparent diameter as the sun, of maximum emissive power, and at the temperature of 1238°C . The temperature of the air was $+1^\circ$, and the barometric pressure 758 millins. In these conditions, the diameter of the admission-aperture being about 25 times the sun's apparent diameter, the portion of the sky bordering the sun, and seen from the

bulb of the thermometer, acted as a surface Ω heated to near 100° , the enclosure being at $9^{\circ}2$. The total intensities of the three radiations sent to the thermometer by the surfaces S , ω , and Ω were then sensibly proportional to the numbers 15, 1, and 0.1.

It will not be uninteresting, and I have already some measurements on this point, to compare at different periods, and especially at different altitudes, the radiation of this portion of the sky bordering the sun, the illumination of which exhibits at times remarkable intensity. Perhaps we shall find there a portion of the heat lost by the direct rays in their passage through our atmosphere.—*Comptes Rendus de l'Acad. des Sciences*, May 18, 1874.

ON A PECULIAR PHENOMENON IN THE PATH OF THE ELECTRIC SPARK. BY PROF. TOEPLER, OF GRAZ.

It is well known that the sparks from the discharge of a Leyden jar leave upon the surfaces of insulators a trace, conditioned by certain mechanical processes. The phenomenon is especially characteristic upon very delicately smoked glass surfaces to which sparks spring between pointed conductors. I have therein observed a regular microscopic structure.

With a length of spark of 4 to 6 centims. the trace is generally a bright streak 3 millims. wide, with a dark axis, produced by the soot-particles being partly thrown to the sides, partly going to the axis and there accumulating. On this trace there is further found a mostly very striking knot-like thickening just where the lateral motion of the air has taken place with peculiar violence—a place in the spark which had already struck me in my optical observations (*Pogg. Ann.* vol. cxxxiv.). Before this spot the trace is altogether different from what it is beyond. Towards the positive conductor the spark-path is mostly branched off like a tuft, towards the negative not so. When the trace is examined with a magnifying-power of 15–20, there appears frequently on the positive side, never on the negative, in the dark axis of the spark-path a very fine dark zigzag line resembling a microscopic sine-curve, of 0.12–0.13 millim. wave-length. From the internal angles of this line issue laterally equidistant bright streaks inclined to the axis of the spark in the direction of motion of the positive electricity. This microscopic structure (the regularity of which is sometimes surprising) is often found also just as distinct on the fine side branches which break forth from the positive part of the spark-path. I remark, further, that the soot-particles which exhibit the structure are in some measure fixed to the glass surface; for when the layer of soot is removed, say, with a fine hair pencil, the dark streak in the axis of the spark remains adhering, though of course the microscopic delicacy of the figure is destroyed.—*Sitzung der math.-naturw. Classe der kaiserl. Akad. d. Wissensch.* in *Wien*, May 15, 1874.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

SEPTEMBER 1874.

XXVI. *On the Opacity of the Developed Photographic Image.*
*By Captain ABNEY, R.E., F.R.A.S., F.C.S.**

IN a series of pictures of the sun which have lately been taken by photography, I found the opacity of the image by no means varied directly as the time of exposure. This caused me to institute an inquiry into the relation of time of exposure and intensity of light on the one hand, and the resulting opacity of the image on the other.

Primarily it was necessary to obtain some known gradation of intensity of light, and then to measure the resulting opacities caused by it on a photographic plate. The gradation was obtained by causing a "star" to revolve rapidly round its centre. The "star" was cut out with great exactness from white cardboard and made with eight "points." The curve of each point was made to take the form of a portion of an equiangular spiral. By this means an arithmetical progression of white was obtained when the star was made to rotate. When revolving in front of a black background, at two inches from the centre of the card (and within that distance) pure white was obtained; whilst at fourteen from the centre pure black was obtained. The black background employed was of such a dead nature that sunlight gave no appreciable shadow on it when an opaque body was placed before it.

The star was made to revolve at the rate of fifty revolutions a second. In some cases a dead-black star was made to rotate before a clear sky, the only access of light being through the openings of the points.

* Communicated by the Author.

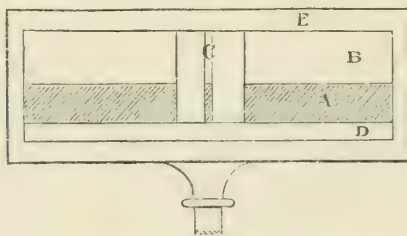
Phil. Mag. S. 4. Vol. 48. No. 317. Sept. 1874.

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Plates were exposed on this object, the negatives being obtained by the ordinary wet process, with simply iodized collodion, an 8-per-cent. nitrate-of-silver bath, and 4-per-cent. iron developer. The strength of the developer was afterwards varied; but for the purposes of these experiments any variation was excluded. Other negatives were obtained on dry plates made with bromized collodion, a 16-per-cent. nitrate-of-silver bath, albumen preservative (washed off, after application, as far as possible), and alkaline development of one particular strength. By alkaline development, as is well known, the bromide of silver is reduced to metallic (or oxide of) silver *in situ*, no free nitrate of silver being applied to the image during development. The opacity of the image obtained by this method is particularly adapted for giving the necessary means of measuring the action of any relative intensities of light acting on the silver for any time.

In order to determine the relative opacities of the image, it was necessary to obtain some standard scale with which to measure. The ordinary methods were tried without success, the image being "matt," or only translucent. Failure with them was inevitable. After various experiments with coloured gelatine wedges, I determined to use coloured glass wedges, and, owing to the kindness of Mr. Browning, obtained three smoke-coloured ones, corrected for refraction by crown glass. These in varying combinations have given me every thing that could be desired. The mounting I adopted for them is as follows.

Fig. 1.



A is the wedge in position, B a space in the frame E, in which any glass whose opacity is to be measured is placed, C a slit, and D a fixed scale dividing the wedge into arbitrary divisions. In actual use the whole of the frame was glazed with finely ground glass, the slit being next to it, and the wedge against that again. When measurements of opacity were taken, the glass to be tested was placed in B and a light placed at a known distance behind the slit. Great care was taken to ensure the equal illumination of C. The length of the wedges are severally 6.5 inches. They do not give a zero of absorption at their thin

ends, it being found necessary in grinding to have an appreciable thickness. I was enabled to calculate the relative absorptive values of each wedge; and the following Table will give an idea of the degree of accuracy with which they were scaled. The values are given in lengths of a half-inch scale, starting from the calculated zero of the wedge which I have called A. Each of the wedges were reduced to the same scale. The numbers refer to different opacities which were measured. A mean of six readings was taken in each case; and in no instance did any reading vary more than .15 from the mean.

	A.	B.	C.
No. I. . .	7.15	7.13	7.13
No. II. . .	10.21	10.20	10.18
No. III. . .	12.44	12.45	12.41
No. IV. . .	17.60	17.50	17.52
No. V. . .	18.60	18.52	18.52

From careful measurements it was found that the coefficient of absorption for each unit of scale of wedge A for the light with which the measurements were taken was .192.

The photographs of the rotating star were taken of the full size of the original, only half of the disk being in some cases on one plate. Strips were cut from these negatives, one edge always passing through the centre of the image of the star. The relative transparencies of every $\frac{1}{4}''$ or $\frac{1}{8}''$ were obtained by comparison with the wedges. From these values the accompanying curves (fig. 2) have been formed, the ordinate being the translucency, whilst the abscissa is a measure of the intensity of the original reflected light. Only four results are shown—two obtained by wet, and two by dry plates. About thirty were measured with almost identical results.

Each strip was compared with the wedge by daylight, and also by an artificial monochromatic light. The results obtained by the one were nearly proportional to those obtained by the other; hence only one curve for each strip is given; and this was obtained by the latter light. To guard against a false ratio of intensity of light due to the lens, negatives of the star were taken at different parts of the plate, and a mean taken. As the lens used was non-distorting and of long focus, the edge and centre of the plate, when directed towards the sky or on a uniformly white surface, had sensibly the same illumination. Each portion of the strip cut from the negative whose opacity was to be compared was placed above the wedge, at B, and opposite the slit C. These were clamped together and moved till light from behind, shining through the slit and through the image and the wedge respectively, appeared of the same brightness on

the ground glass. The position of the slit in regard to the scale was noted, and the intensity of light transmitted calculated by the ordinary formula. Each strip was compared six times—three times by myself, and three times by an assistant. A mean of the six readings was taken as correct.

Fig. 2.



A and B are the curves given by the images on the dry plates.
C and D are the curves given by the image on wet plates.

The dotted lines indicate the line whose ordinates give an arithmetic progression of transparency, E F being unity or transparency.

FG represents the length of the strips examined, and therefore the varying intensity of light, F being zero and G the maximum.

Regarding the curves given by the dry plates, if we suppose that varying intensities of light cause a corresponding reduction of the bromide of silver after development, it can be easily demonstrated that the intensity of light passing through the image after clearing away the unaltered bromide would be

$$I' = n \cdot \epsilon^{-kI}, \quad \dots \dots \dots (\alpha)$$

where n and k are constants depending on the thickness and opacity of the bromide film, and I the intensity of the light *producing* any one part of the image—that is, on the supposition that the image is formed of matter continuous but of varying density. This is not the case, but there is an approximation to it. Under the same supposition we can assume that there is a function of time into a function of intensity of light acting on an infinitely thin layer of the bromide of silver which will cause an entire reduction of the bromide on development: this we might call a state of saturation. In the image of the star there may be some point where the upper layer of bromide (of infinite thinness) is saturated. From that point along the image to be produced by the higher intensities the whole surface is saturated, and the saturation must gradually approach the bottom surface. From the point where the whole depth of the layer is saturated, along the image to be produced by still higher inten-

perature approaches the boiling-point, but returns on cooling. Moreover this operation may be repeated without the substances suffering decomposition. Cudbear, camwood, logwood, and turmeric are selected as illustrations of the properties cited.

Cudbear yields a brilliant orange fluorescent light, and is visible in diffused daylight without the agency of a condensing lens, which is necessary to show it in an alcoholic solution.

Camwood exhibits a powerful apple-green fluorescence, although wholly destitute of this property in aqueous or alcoholic media. The spectrum of the fluorescent light is continuous from E downwards, interrupted by two narrow faint shadings situated at $3\frac{5}{8}$ and 5 of Sorby's scale.

With regard to logwood, unless the castor-oil solution be saturated, sunlight and a lens are requisite to bring out its fluorescent character. The colour very much resembles that of camwood, but is distinguished by its spectrum, which is continuous from *b*, but interrupted by two shadings at $4\frac{1}{4}$ and $5\frac{3}{4}$.

Turmeric is well known to fluoresce powerfully in alcohol a yellow-green, and in benzole a blue-green. In castor-oil, however, the fluorescent light is at least three times as bright as in other fluids, and may be described as a vivid emerald-green, evident in the duldest daylight; but if a flat bottle of the solution be placed on black velvet behind rather deep cobalt-glass when the sun is shining, the phenomenon is of a most brilliant description, and without exaggeration may be compared to that produced by the beautiful uranium-glass. The spectrum furnished by the fluorescent light is characterized by transmission of red and green rays, and blue to F, with a faintly perceptible shading at the yellow end of the green.

These facts therefore show that, in studying the phenomena of fluorescence, advantage should be taken, whenever possible, of this valuable solvent property of castor-oil.

XXVIII. *The Constant Currents in the Air and in the Sea: an Attempt to refer them to a common Cause.* By BARON N. SCHILLING, Captain in the Imperial Russian Navy.

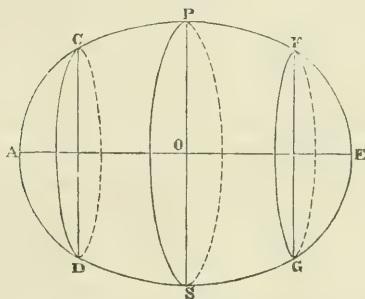
[Concluded from p. 169.]

AS we are speaking of wave-motion, it will not be out of place to mention here a circumstance which will subsequently be of importance for our argument.

It is that the theory of waves, which is commonly laid as a foundation for all tidal phenomena, has called forth two views which cannot possibly be both together correct. In the first place, it is generally assumed that the flood tide rises just as far

above the ordinary sea-level as the ebb sinks below it. Secondly, it is assumed that the middle time between high and low water corresponds to the normal level. The highest water is formed by the two cusps A and E (fig. 3) of the tidal ellipsoid A P E S,

Fig. 3.



and the lowest by the circle P S, which halves the surface of the ellipsoid at its minor axis. The normal level will therefore, according to the common assumption, be found on the circles D C and F G, which run parallel with the circle P S and are distant 45° of arc both from the points A and E and from the circle P S; so that $P F = F E$ and $P C = A C$; that is, about three hours after flood the normal level, and three hours later the ebb comes in. On this assumption, however, the superficial space of the surfaces A C D and E F G together, occupied by the flood tide, is $2\frac{1}{2}$ times as small as the superficies of the middle zone C F G D, in which the water stands at the ordinary level. But since the water which forms the accumulation of the flood can only be derived from the ebb-zone, it is clear that, on this assumption, the same mass of water must rise considerably more on the smaller space than the water-surface sinks in the ebb-zone. If, on the other hand, we adhere to the assumption that the water rises as high above the normal level as it sinks below it, the surface occupied by the two floods must be just as great as that occupied by the middle ebb-zone, and the two circles at which the normal level is found must be only 30° distant from the central circle, but 60° from the cusps A and E of the ellipsoid. Flood tide would thus last eight hours, but ebb only four. Or the water must fall as much in the last two hours of its going down as in the first four after high water, and likewise rise as much in the first two hours after its lowest as in the remaining four. Probably the reality lies between the two assumptions; that is, the rise of the water during flood is probably more considerable than its fall during ebb, and, on the other hand, the

circles of normal level are more than 45° and less than 60° of arc distant from the cusps of the tidal ellipsoid*.

As at coasts the currents produced by the flow and ebb are always observed to flow alternately in precisely opposite directions, it is generally believed that the attraction of the moon and sun cannot exert any influence on the constant currents. Mühry says, "It is scarcely necessary to mention that the tidal motion, which daily carries its two meridian waves round the globe, is something altogether different from the rotation-current: the former extends over all latitudes, and generally occasions no forward motion of the mass of water, but only waves, *i. e.* oscillations. . . . Such an assumption is contradicted also in a peculiarly decided manner by the return-currents flowing on both sides of the equatorial current in a wide semicircle from west to east (therefore against the tide-wave)—the compensation-arms of the rotation-current, which at the same time enclose each a wide central space filled with still water and floating seaweed, the Sargasso-seas. How can the tide-wave call forth such phenomena? We are of opinion, moreover, that, if there were no moon, the equatorial current would still exist while the earth revolved on its axis; but it would not exist if the globe did not turn on its axis, even though the moon should daily travel round the earth"†.

We cannot possibly share this opinion of Mühry's. We will besides let the thing speak for itself, subjecting the action of the attraction of the sun and moon to a closer consideration.

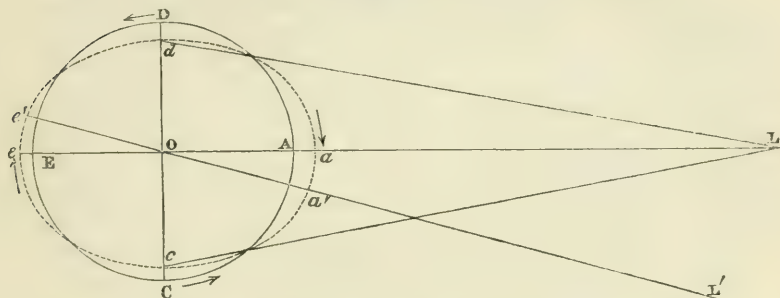
Suppose the circle $ACED$ (fig. 4) to be the equator, and L the centre of the moon, which we will imagine in the plane of the equator. If, then, the earth had no rotation, the surface of the sea must take the form of the dotted line $aced$. To form this ellipsoid, currents must proceed from all sides towards the cusps a and e , lasting until the ellipsoid had attained its due elongation. But since the earth is constantly turning, the moon relatively to the earth will have already arrived at another point before the water and the atmosphere have had time to properly form the ellipsoid $aced$. Of course the currents will immediately direct their course to the new point of attraction; and since this again alters its position, a current must be produced in the air and water which must endeavour to follow the motion of the moon and shift the cusps of the ellipsoid perpetually from east to west. On the other hand, by the shifting of the moon

* It appears, therefore, that the zero-point of the tide-gauges has not yet received its true position. This must lead to erroneous results in levelling-surveys when the heights of two neighbouring seas are to be compared in which the heights of the tides differ (as, for instance, Panama).

† Mühry, *Ueber die Lehre von den Meeres-Strömungen*, p. 9.

from L to L' , all the points in the arc $ec a'$ are moved somewhat nearer to the moon, and therefore the attraction of the moon on

Fig. 4.



all these points is increased; while every point in the arc $e' d a$ has removed a little further from the moon, and is consequently less attracted. We will represent the attraction of the moon by two threads Lc and Ld fastened to the circle. We will gradually more and more draw the thread Lc , to represent the constantly augmenting attraction of the point c . We will constantly let the thread Ld give way, to imitate the diminution of the attraction of the point d . Of course, through greater tension of the thread Lc and continual yielding of the thread Ld , the points c and d receive a motion in the direction of the arrows C and D . This motion will be the quicker the greater the circle to which the points belong, because in greater circles the change of distance from the moon, and consequently the alteration of her attraction, is more considerable for every point than in smaller circles.

All that we have just said of the moon holds good also for the sun, with only this difference—that the motions of air and water produced by its attraction will be somewhat less than those produced by the moon.

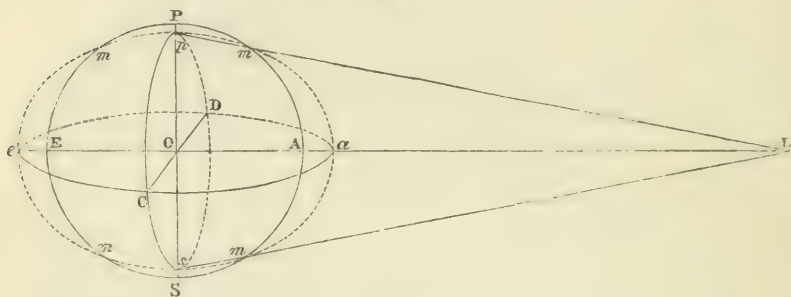
We see therefore that the attractions of the sun and moon must each present two reciprocally counteracting developments of force. The one, which calls forth an east-to-west current and corresponds to high water, we will henceforth name the flood-current force; the other, corresponding to the ebb and impelling air and water from west to east, we will call the ebb-current force.

If these two forces are of equal intensity, they will balance each other and produce no current; but as soon as one of the two is greater, the water and air will be subject to the action of the greater force and move onward with the velocity corresponding to the difference between the two forces.

Before we compare, however, with one another the quantities of these two forces, it will be necessary to illustrate further what has been said, representing the earth in the plane of the meridian.

Let the circle $P A S E$ (fig. 5) be a terrestrial meridian, and

Fig. 5.



L the centre of the moon (or of the sun), which, as before, is on the equator. The dotted line $p a s e$ marks the form of the tidal ellipsoid. Through the rotation of the earth the moon apparently moves from east to west; with it the ellipsoid $p a s e$ turns about the axis $P S$, and develops, as we have seen, at the equator two forces opposed one to the other. The one, the force effecting the flood-current, directs its course from east to west, and in the case here given is strongest at the equator, on which the cusps of the ellipsoid move forward as long as the moon is on the equator. This force will act in nearly the same direction on both sides of the equator; only it must rapidly diminish as the latitude increases; and in the latitude of the points m , where there is no rise of the water, the force acting from east to west must be $=0$. Further polewards the tendency to form the tidal ellipsoid may probably develop an inconsiderable current from the pole towards the equator, as shown by the lines $p L$ and $s L$.

The ebb-current force acts from west to east, as if the circle $p C s D$ revolved in this direction on the axis $P S$. As already said, it arises from the circumstance that all points in one half of the earth are brought nearer to the moon by the rotation, while all those in the other half are carried further from it. The ebb-current force has its greatest intensity at the equator, and diminishes very gradually on both sides of it, since the parallel circles in low latitudes become only gradually smaller. Only in high latitudes, where the circles diminish rapidly, does the force of the ebb-current quickly diminish; and only at the poles does it entirely cease.

Since, as we have shown, the flood rises more above the nor-

mal level of the sea than the ebb sinks below it, we think we can assume, as an hypothesis, that the force of the flood-current will also be greater than that of the ebb-current.

In our case, if the cusps of the ellipsoid are on the equator, and therefore both forces develop their maximum on that circle, the greater force must overpower the smaller, and both in air and sea a current from east to west must prevail all along the equator. On both sides of the equator the force of the flood-current, acting from east to west, diminishes rapidly polewards; the counteracting force of the ebb-current diminishes more slowly. Therefore, at a certain distance from the equator, the greater but rapidly diminishing force directed from east to west will be only just as great as the smaller only slowly decreasing force directed from west to east. In these parallels the forces, balancing each other, will generate no current. Still further polewards the force of the flood-current, still continually more decreasing, will be less than that of the ebb-current, and, both in the sea and in the atmosphere, currents from west to east will make their appearance. In the latitude of the points *m* the east-to-west force ceases entirely; while the opposite force has in this latitude lost only a small portion (less than half) of its action, and hence may here produce a considerable current. In higher latitudes the force of the ebb-current will also quickly diminish, and the currents from the west become considerably less, and their direction probably turn more towards the equator. Accordingly, in the northern hemisphere, in high latitudes, currents will arise from the north-west, and in the southern from the south-west.

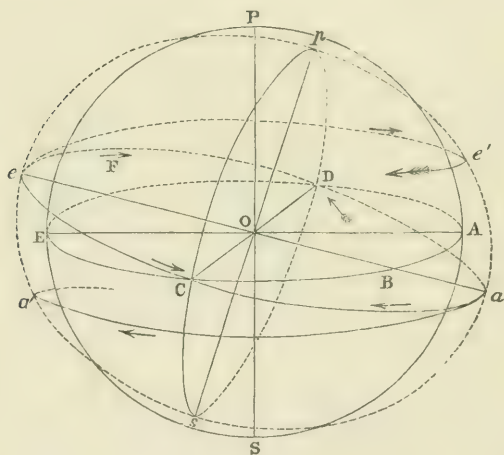
When, therefore, the moon and sun are at the same time in the vicinity of the equator, a current in air and sea must flow there from east to west. On both sides of the equator this current will diminish polewards until it entirely ceases; and there must thus be produced a streamless zone parallel to the equator. Further polewards a west-to-east current will prevail, which must at first increase gradually until it attains its maximum; then will this current also again diminish gradually, and in high latitudes flow from the north-west in the northern hemisphere, and from the south-west in the southern.

In reality we find this to be the constitution of the currents. In middle latitudes constant west winds and sea-currents directed eastwards prevail. In the latitude of about 30° there is in each hemisphere a zone of no current, and in the tropical regions we find currents flowing perpetually from east to west, both in air and sea. An apparent exception is, that on the equator we meet with a zone in which no current is perceptible either in the atmosphere or in the ocean.

This circumstance seems at the first glance to contradict the theory of the moon's attraction; yet the origination of this equatorial streamless zone is easily explained when we reflect that the moon and sun are simultaneously in the vicinity of the equator only for a very brief time twice yearly. They usually describe parallel circles which lie between the equator and the tropics; the moon only goes sometimes slightly beyond the last-mentioned circles. The ellipsoid arising from the united attractions of the moon and sun must always have its cusps between the sun and moon; and hence these cusps must mostly describe parallels between the equator and the tropics.

Supposing the tidal ellipsoid in the position *a sep* (fig. 6),

Fig. 6.



its major axis ae making a certain angle with the equator $ACED$, by the earth's rotation on its axis PS the cusps a and e of the ellipsoid will describe the parallel circles aa' and ee' ; and therefore the maximum of the flood-current will also be observed on these parallel circles. The current will also not preserve its exact east-to-west direction, but, as shown by the arrows B and F , come from E.S.E. in the southern hemisphere, and from E.N.E. in the northern.

On both sides of the parallel circles aa' and ee' the force of the flood-current will, as already said, diminish rapidly; while the ebb-current keeps, as before, the maximum of its force at the greatest circle, therefore at the equator, and, also with this position of the ellipsoid, diminishes only slowly polewards, con-

sequently will have already become slightly less at the parallel circles $a'a$ and $e'e$, on which the maximum of the flood-current is found. The direction of this current will also not be purely from west to east, but, as the arrows C and D show, alternate between W.N.W. and W.S.W. The opposite forces of the ebb- and flood-current must therefore on both sides of the parallel circles $a'a$ and $e'e$ balance one another and form a zone of no current.

This appears to occur in the vicinity of the equator and of the parallels of 30° latitude, the zones of calms and of the Sargasso-seas being found there. In the latitudes of the parallel circles $a'a$ and $e'e$ must be the maximum of the east-to-west flood-current; this perfectly corresponds with the phenomena of the trade-winds and the equatorial currents. Polewards from the streamless zone in the 30th parallel of latitude the rapidly diminishing force of the flood-current must be overpowered by that of the ebb-current, and a constant current from west to east be produced—which also actually happens; for between the 40th and 50th parallels of latitude, or thereabouts, both in the air and in the water, in all oceans and in both hemispheres, a current directed eastwards is constantly observed.

Hence, it seems to us, the action of the attraction of the sun and moon explains the origination of the trade-winds and anti-trades with their zones of calms, and the rotation-currents running parallel with the equator, with the Sargasso-seas and the streamless equatorial zone, considerably better than all hitherto existing hypotheses.

If our explanation of the trade-winds and equatorial currents is correct, also the position and the breadth of the current-zones and the strength of the currents must themselves depend entirely on the position of the tidal ellipsoid or on the position of the moon and sun with respect to one another and relative to the earth. When, for instance, moon and sun are both very near the equator, the equatorial calm-zone must be non-existent, the calms of the tropics must approach towards the equator, and the constant west winds blow with greater force in lower latitudes. Whether all this happens is unknown to us; yet strong west winds usually rage in Europe at the times of the equinox. Just so, perhaps, it sometimes happens that ships cross the line without calms; but whether this chiefly coincides with the time when the moon crosses the equator we know not.

When moon and sun are at the same time in the vicinity of the tropics, the current-zones must be displaced polewards, and the equatorial calm-zone be especially broad. It is possible that then the ebb-current may predominate in the middle of the zone, and that this circumstance accounts for the west-to-east current

which flows in a narrow band along the equator and is named, in Berghaus's 'Chart of the World,' the "equatorial counter-current." In the air this current does not exist. It would therefore have to be ascertained if this equatorial counter-current is a constant one or is only to be observed when the moon approaches the tropics, and whether it is not wanting when the sun and moon are simultaneously in the vicinity of the equator.

The shifting of the trade-wind zones appears to be on the whole more considerable than that of the sea-currents, and seems in many cases to coincide with the change of the seasons of the year—which, then, proves that the sun by its heat also exercises an influence on the trade-winds. This probably takes place chiefly through the sun's action on the aqueous vapour in the atmosphere and through other collateral circumstances. The main cause, however, of the production of the trade-winds must certainly be ascribed to the attraction of the moon and sun; and hence their position relatively to each other must have a sensible influence upon various atmospheric phenomena. It appears, therefore, possible that the well-known old popular tradition of the phases of the moon affecting the change of the weather may have some foundation; only it might be more correct to ascribe this influence not to the phases, but to the distance and declination of the moon, which latter, it is true, stands in a certain connexion with the moon's phases and the sun's declination. At the times, namely, of new and full moon the difference between the declination of the moon and that of the sun is always inconsiderable, although at the time of full moon the sun and moon are in different hemispheres but at nearly equal distance from the equator.

Only at the time of the quadratures can the difference of declination of the sun and moon be considerable; at the periods of the equinoxes and the solstices the difference rises at the utmost to near 28° .

From the production of currents by the moon's attraction not only can the sea-currents parallel to the equator, but also the meridional currents be naturally derived.

If the whole earth were covered with water, the equatorial current would flow round it unhindered; but now the continents stand as insuperable obstacles in the way of this motion. As, however, the cause of the flow is not hereby removed, the current in the ocean must continue and cannot suddenly cease on impinging against the coast, but must change its direction according to the position of the shore. Thus we see, in the Atlantic Ocean, that the southern equatorial stream divides at Cape St. Roque (which opposes it like a wedge), and, following the direction of the coast, is turned aside, part to the north-west

and part to the south-west. The north-west branch of this current unites in the Caribbean Sea with the northern equatorial, and in this way impels almost the whole of the warmed water of the surface of the Atlantic equatorial zone into the Gulf of Mexico. The great mass of this warmer and therefore lighter water driven together by the equatorial current must, of course, have the tendency to spread over the colder and heavier water, and to flow off northward. Thus arises, then, a current of warm water flowing out of the Gulf of Mexico, commonly known by the name of the Gulf-stream.

The motive force of the Gulf-stream must therefore be derived partly from the pressure of the equatorial current, partly from the tendency of the warm water to spread over the cold of the higher latitudes, but partly also from the attraction of the current, directed eastward, of the middle latitudes; but all these causes spring directly from the attraction of the sun and moon, which thus must be regarded as the prime motive force of the Gulf-stream*.

The eastward current of the middle latitudes and the north-east movement of the entire northern portion of the Atlantic Ocean form the continuation of the Gulf-stream, and hence are often designated by the same name—to which we have no objection, if it be kept in view that the prime cause of motion of the two last-mentioned currents lies in the force of the ebb-current. As already said, in about 30° latitude this force commences to overpower the force of the flood-current, and develops the maximum of its effect somewhere between the 40th and 50th degrees of latitude; further polewards it diminishes considerably, and becomes so feeble that it is no longer perceived as a current. Nevertheless a slight movement eastwards appears to extend considerably further towards the pole, and gradually to collect the warmer water on the coasts of England and Norway. This warmer water is derived partly from the Gulf of Mexico; but part of it may have been heated on the surface of the ocean in higher latitudes. The ebb-current, therefore, collects the superficial warmer water in the eastern part of the ocean; and the tendency of the warm water to spread over the colder impels it north-eastward, and thus accounts for the motion of the northern portion of the Atlantic.

The principal force of the ebb-current, flowing eastward, is deflected south by the coasts of Europe, and, following the coast of Africa, returns again into the equatorial stream. The attraction of the latter perhaps forms the principal cause of the south-

* Self-evidently it is not meant that the sun and moon's attraction heats the water of the Gulf of Mexico; but it is that which generates the equatorial current and thus collects the warm water in the gulf.

ward bend, but may be assisted in some degree by the tendency of the particles to move towards the equator, produced by the rotation of the earth. Only a small portion of the east-directed current passes Cape Finisterre unhindered, and continues its course in the natural direction along the north coast of Spain till the coast of France compels it to curve sharply to the north-west and follow exactly the course of the shore of the Bay of Biscay, under the name of the Rennell current, to be lost at the English coast in the general north-east current of the Atlantic. The Rennell current shows distinctly how much power the direction of coasts has to determine that of currents, even to reverse their direction.

A portion of the South-Atlantic equatorial current turns to the south-west from Cape St. Roque, along the coast of South America. The impelling force of this Brazilian current is the same as that of the Gulf-stream—partly the pressure of the equatorial, partly the high temperature of the water heated in the Atlantic Ocean and collected at the coast by the equatorial current, and partly the attraction of the eastward-directed ebb-current functioning in the middle latitudes, into which the greater portion of the Brazilian current passes to form the South-Atlantic rotation-current. This latter, after crossing the ocean from west to east, and having curved a little to the north, strikes upon the African coast, and (for the same reasons as those above discussed for the northern hemisphere) returns along it again to the equatorial current, forming the South-Atlantic Guinea current. The entire rotation-current, then, is originated by the attraction of the moon and the sun, as this by its direct action carries the water in the equatorial regions from east to west, and in the middle latitudes from west to east, and hence also generates indirectly the currents flowing in the direction of the meridian (the Gulf-stream and the North-African current, the Brazilian and the South-Guinea currents).

In the entire southern hemisphere all the cold polar currents are directed north-east, which coincides perfectly with the action of the moon's attraction in higher latitudes. Only in the northern hemisphere the directions of the cold polar currents contradict the laws of the moon's attraction; for the Greenland current and the cold current of the Japanese sea have a south-west direction, and not a south-east one, which they should have according to our considerations. This, however, may well have its cause in the action of the ebb-current, directed from west to east, which gradually withdraws the warm northward-flowing current from the coast; and this is replaced partly by the cold water of the bottom, but principally by the less-salt and therefore lighter water derived from the melting of the ice. A similar

phenomenon is often produced at coasts by the action of the wind; and those who have sought a sea-bath will remember that with a land-breeze the water is always colder than with a sea-breeze. The former removes the warmed superficial water from the coast, by which the colder water beneath is brought to light. The sea-breeze, on the contrary, drives to the shore the water which has been warmed on the surface of the sea. What the wind does in this case may well be brought about in a higher degree by a permanent sea-current. In the depths, even in the northern hemisphere, the polar currents appear to be directed to the south-east. This is demonstrated by the many icebergs which, near Newfoundland, cut through the Gulf-stream in that direction. Dana's chart of the isothermal lines of the sea-surface in the coldest month*, on which the distribution of the corals is given, permits us also to draw a similar conclusion. The polar limit of the coral zone, both in the Atlantic and in the Pacific, is (probably on account of the water being too cold) about 10° nearer the equator on the east side than on the west side of the same ocean. It is interesting that, according to this chart, the northern boundary of the corals is 10 degrees more to the north in the Pacific than in the Atlantic Ocean. The reason is probably to be sought in the fact that the Atlantic forms almost the only discharge, and the main supply, of the north polar basin.

The alternating warm and colder strips of water in the Gulf-stream, as well as in the Kurosiwo, seem to us to favour the idea that the force which carries away from the coast the entire current eastwards is not constantly of equal strength, but, so to speak, has a reflex action—which perfectly corresponds with our hypothesis, according to which, in the middle latitudes, the force of the ebb-current must on the whole take the upper hand, but, through the westward-directed force of the flood-current, may be subject to periodical interruptions.

L. von Schrenk, Member of the Academy of Sciences of St. Petersburg, has recently, in a very interesting work (*Strömungsverhältnisse im Ochotskischen und Japanischen Meere*), pointed out that in the Yellow, as well as in the Japanese and partially in the Ochotsk Sea, the temperature of the water is constantly lower at the east coasts of the continent and the islands than at the west coasts. We see in this a proof that in these inland seas there is the same tendency of the water to move eastwards, and that thereby the upper warmer water is accumulated at the east side of the sea or at the west coast of the land. In the White Sea also, and the Varanger Fjord in North Lapland, the temperature of the water is higher in the eastern parts than

* Stieler's *Hand-Atlas*, 1867, No. 9, Carton.

in the western. As we have already remarked, the warmer water accumulated on a coast must flow away polewards, while the cold water of the west side of the sea seeks to occupy the space left free, and so flows towards the equator. It is also interesting that Schrenk* has pointed out the existence of strips of cold water in the warm current of the Japanese Sea. The colder but very slightly less salt water may, under some circumstances, have exactly the same specific gravity as the warm, somewhat saltier water; and hence they may flow a long time side by side without mingling. These strips of colder water have not yet been demonstrated in the Brazilian and Mozambique currents; but it is probable that they are present there also, especially in the Brazilian current, which extends further south. Indeed it is likely that these warm currents are separated from the coast by colder water.

The Mozambique current, it seems to us, strikingly corresponds with the theory of the moon's attraction. It has its origin in the equatorial stream of the Indian Ocean, then follows the east coast of Africa in a south-westerly direction, and, still following the coast, at the southern extremity of the continent takes a westward direction, but thereby comes into the region of the ebb-current and at once, with a remarkably sharp bend, returns eastward. We can only account for this sudden flexion by the action of the moon's attraction; for it is impossible to admit that the aspirating force of the Indian equatorial stream can occasion this sudden bend in order to carry the Mozambique current to the shores of Australia and New Zealand. Moreover the depth to which the constant ocean-currents extend appears to us to be explicable only by the attraction of the moon and the sun; for it acts on all the water-particles as far as the bottom of the ocean, if its action below is slightly less than its action above. The currents of the remaining oceans are so perfectly similar to those above discussed, that in describing them we should have to repeat nearly the same things. They are all originated principally, either directly or indirectly, by the action of the flood- and ebb-currents, and hence can only be satisfactorily explained by that action.

The currents of the atmosphere rest at all events upon precisely the same laws; but air-currents are far more susceptible to all possible collateral causes than ocean-currents, and are therefore subject to many other influences, amongst which difference of temperature plays a certain part. Unfortunately this influence has hitherto been considerably overrated; for polar and antipolar currents generated by difference of temperature

* *Op. cit.* p. 56.

have been regarded as the basis of meteorology, or as currents on which all the movements of the air depend. To this opinion we cannot assent; on the contrary, we believe that in the atmosphere, just as in the sea, the principal motions take place in directions nearly parallel to the equator.

Perhaps, in the future, with more accurate knowledge of the action called forth by the attractions of the sun and moon, we shall succeed in explaining the causes of rotatory storms by the two opposite directions of the flood- and ebb-current. May not in certain cases, at the time of the quadratures, the ebb-current caused by the moon meet at a certain angle the flood-current called forth by the sun and thereby produce the rotating motion? Up to the present time the important natural phenomenon of cyclones has by no means been explained; for all hitherto-given explanations have been quite inadequate.

As is known, these storms always have two motions—one rotating, and one progressive. The progressive motion corresponds well with the theory of the moon's attraction; for these storms almost always commence in low latitudes, and, in both hemispheres, the centre of the storm moves westward in the region of the flood-current, at the same time slightly increasing its distance from the equator and thereby arriving in the calm-zone of the tropic. Here the velocity of the progressive motion becomes considerably less, and its course makes a sharp curve eastward, the hurricane passing into the region of the ebb-current; and now, in both hemispheres, it moves with great velocity to the east and somewhat polewards. Therewith its diameter gradually increases and the circular motion diminishes until the hurricane is lost in higher latitudes. The usual duration, from beginning to end, of the hurricane is about 14 days.

The rotating motion of these storms is subject to quite determinate but not yet discovered laws. In the northern hemisphere they rotate in the opposite direction to that of the hands of a clock; but in the southern hemisphere they go round in the same direction as the latter. In other words, in both hemispheres the storm always blows from the west on the side towards the equator, and from the east on the polar side. Westwards of the centre of the hurricane, the direction of the storm is always to the equator; eastward of the centre, away from the equator; so that hurricanes rotate in an opposite direction to the currents of the seas. Ordinary storms appear to stand in the closest connexion with cyclones; at least this conjecture is corroborated by Buys-Ballot's law, according to which the winds revolve about the minimum of atmospheric pressure in the same direction as the cyclones.

The explanation that the rotating motion of cyclones arises

from the rotation of the earth is altogether inadmissible; for the hurricane always commences in very low latitudes, with a diameter which seldom occupies more than 2 or 3 degrees of the meridian. The difference in magnitude of the parallel circles, however, is so inconsiderable that the air streaming to the centre can only be deflected by the earth's rotation to an angle of 2 or 3 degrees from the meridional direction. Assuming that the centre of the cyclone is, at the beginning of the hurricane, in 10° latitude, that its radius occupies 2 degrees of the meridian, and that the air requires two hours in order to traverse this distance, and retains during the whole time the rotation-velocity of the parallel circle which it has left behind, in this case the air-particles from the 12th degree of latitude, streaming to the centre of the hurricane, would deviate a little to the west from the meridian, forming with it an angle of $2^\circ 45'$. Those from the 8th degree of latitude, streaming to the centre, would deviate eastwards, their direction forming with the meridian an angle of $2^\circ 21'$. But this much too small deviation from the meridian cannot possibly occasion the rapid whirling motion of the storm.

Not doubting that such a theory of the ocean-currents and the trade-winds, founded on the attraction of the moon, may be the correct one, we nevertheless acknowledge how much our view needs to be subjected to further elucidation. Time must bring a multitude of fresh observations before the special authorities can have spoken their last word on this subject. To us, however, it will afford the fullest satisfaction if we have had the good fortune, by the foregoing analysis of our views, to contribute, at least indirectly, to the advancement of this department of physical geography, which has hitherto wanted a uniting fundamental idea.

XXIX. *The Hydrodynamical Theory of the Action of a Galvanic Coil on an external small Magnet.*—Part I. By Professor CHALLIS, M.A., F.R.S.*

1. **T**HE mathematical theories of the physical forces which I have published from time to time in this Journal have been made to rest exclusively on the following hypotheses:—All visible and tangible substances consist of inert spherical atoms of constant magnitude, and all physical force is either mode of pressure of the æther on the surfaces of the atoms, or reaction of the atoms at their surfaces due to the constancy of their form and magnitude. The æther is supposed to be a continuous elastic substance, filling all space not occupied by atoms, of

* Communicated by the Author

perfect fluidity, and of the same density every where when at rest, and when in motion varying in density always and at all points in exact proportion to variations of its pressure. Also the size of the atoms is supposed to be so small that even in dense bodies they fill a very small portion of a given space.

2. These hypotheses, which I have enunciated on several previous occasions, are repeated here for the purpose of directing attention to what especially characterizes them. *They involve no assertion that is not comprehensible by the indications of common sensation and experience.* It is because they possess this character that the physical theories I have founded on them differ from those generally maintained by contemporary physicists, which rest for the most part on experimental data conjoined with arbitrary hypotheses not in the same manner intelligible. It does not, however, follow from the dissimilarity of the hypotheses that the two modes of philosophizing are contradictory to each other. This I think I shall be able to show by pointing out the distinction between their fundamental principles, and the consequent relation in which they stand to each other.

3. For this purpose reference will be more particularly made to the physical theories of *magnetism* and *galvanism*, as proposed by Gauss and Ampère, or illustrated and extended by other physicists who have adopted their views. The object of all investigations of this class is to deduce from the results of certain fundamental experiments, by the intervention of arbitrary or provisional hypotheses, mathematical expressions of the *laws* of the phenomena. Accordingly natural philosophy is not thereby advanced beyond a stage analogous to that to which physical astronomy was brought by the results of Kepler's observations. Newton's hypothesis of a gravitating *force* varying inversely as the square of the distance, and his discovery of the mode of calculating its effects by mathematics, were steps necessary for completing that science, inasmuch as they gave *reasons* for Kepler's laws. In the empirical theories I am referring to, the consideration of physical force is included, and from certain hypothetical modes of action of the forces mathematical expressions of the laws of the phenomena are deduced. But confessedly the intrinsic nature of the forces, and the reasons for the facts and hypotheses on which the investigations rest, are left undetermined.

4. The final stage of physical investigation is reached when explanations of phenomena and of their laws have been given by means of mathematical deductions from hypotheses satisfying the condition of being intelligible from sensation and experience. Till this is done, we can hardly be said to have arrived at *theory*

properly so called. The antecedent steps of theory ought, for distinction, to be called *empirical* or *provisional*, as resting on arbitrary hypotheses, and as subsidiary to true and complete theory. True theory rests on hypotheses that are not only comprehensible, but also *ultimate* and *necessary*—that is, such as do not admit of being accounted for by any ulterior hypotheses. This will be proved to be the specific quality of the hypotheses stated above (art. 1), if they should be shown to be adequate to the explanation of the nature, laws, and consequences of the operation of all the different kinds of physical force. To demonstrate their adequacy for this purpose has been the express object of the many theoretical researches I have been occupied with relative to the *modus operandi* of physical force generally. This course of philosophy I propose to call *Newtonian*, its “foundation” having been indicated by Newton in the Third Book of the *Principia*. (See the discussion of this view in the *Philosophical Magazine* for October 1863, p. 280.)

5. Having thus pointed out that a distinction is to be made between empirical theory resting on arbitrary hypotheses and ultimate theory resting on strictly *à priori* hypotheses, I have further to state in what respect the two kinds of theory may be considered to be related to each other. Let it be supposed that by means of a theory depending on certain ascertained facts, and on hypotheses thereby suggested, a true mathematical expression of the laws of the phenomena proposed to be accounted for has been obtained. According to views entertained by some theorists of the present day, natural philosophy consists in thus arriving at phenomenal laws, and there is no occasion for any further investigation. But the principles of the philosophy I call “Newtonian” demand that the explanations of all phenomena and their laws should be inferred by mathematical reasoning from the before-mentioned fundamental hypotheses. Now this may be done in two ways—either directly, by independent deductions from the *à priori* hypotheses, or intermediately, by deducing from the same hypotheses explanations of the facts and hypotheses which form the basis of a true empirical theory. It is evident that in either way the phenomena are shown to be consequences of the operation of intelligible causes, and are completely explained. It appears thus that the empirical method is subsidiary to the *à priori* method whenever the explanation of phenomena is effected by the aid and intervention of the former, and that in this respect the two methods may be mutually related. These remarks will receive elucidation in the course of the subsequent discussions.

6. I propose, in the first instance, to give an *à priori* explanation of the facts relating to the action of a large magnet on a

small needle from which, by the intervention of certain arbitrary hypotheses, Gauss inferred the law of the inverse square in magnetic action. For this purpose it will be convenient to refer to the detailed exhibition of Gauss's argument given in the *Astronomer Royal's 'Treatise on Magnetism'* (Macmillan and Co., 1870). I have already discussed this question on hydrodynamical principles in a "Note on the Hydrodynamical Theory of Magnetism" contained in the *Philosophical Magazine* for July 1869, to which I beg to refer for details of the mathematical reasoning relating to the physical conditions of magnetic force. I propose to reproduce here only so much of that discussion as may be required for understanding the subsequent theory of the action of the galvanic coil on a small magnet, which is the ultimate object of the present communication.

7. In the article just referred to it is assumed that in a magnetized bar there is a small and regular increment of atomic density from end to end, like that which must be produced by the action of gravity from the top to the bottom of a solid or fluid mass resting on a horizontal plane. In a "New Discussion of the Hydrodynamical Theory of Magnetism," contained in the *Philosophical Magazine* for June 1872, I have proved (in arts. 4-9) that if *any* body in which such gradation of density exists be traversed either by a steady ætherial stream, or by a uniform series of undulations of the æther, a *secondary steady stream* will be produced by impulses continually given to the fluid in the direction from the rarer to the denser parts of the body, this being the direction of the contraction of channel by the occupation of space by the atoms. The application of this result forms an essential part of the hydrodynamical theories of electric and magnetic attractions and repulsions which I have proposed and discussed in several previous communications. In the case of a magnet, the gradation of atomic density, when once induced, subsists independently of the action of an external body, and is consequently maintained by the intrinsic molecular forces of the magnet itself. Accordingly I have assumed that whereas in general the molecular attractions acting on a given atom in equilibrium counteract each other, as do also the molecular repulsions, in the case of a magnetized steel bar the equilibrium of the atom results from an equality between the molecular attraction towards the denser end and the molecular repulsion towards the rarer end. This, in short, is considered to be the distinctive property of a substance susceptible of being magnetized. Steel possesses this property in an eminent degree, and can be permanently magnetized. Soft iron admits only of temporary magnetization.

8. The magnetic state of a substance being thus *defined*,

and its magnetic action being supposed to be attributable to the ætherial streams which, as indicated above, this state generates when the substance is traversed either by steady streams or a uniform series of vibrations, we have next to inquire respecting the origination of these primary movements of the æther. I thought, at first, they might be due to the ætherial streams which relatively pass through atomically constituted substances in consequence of the earth's revolution about its axis and motion in its orbit, and of the motion of the solar system in space. But since in that case the primary, and by consequence the secondary, motions would be subject to large fluctuations of intensity to which there is nothing corresponding in the phenomena of a magnet, it follows that the streams which are the exponents of magnetism cannot be to any sensible amount due to the above-mentioned primaries, and must have a different origin.

9. Having proved, as stated in art. 7, that the secondary streams might be generated by a uniform series of ætherial undulations, and having repeatedly maintained (in articles in the *Philosophical Magazine* and in my work on the *Principles of Physics*) that attractions and repulsions may be attributed to the dynamical action of such undulations on the individual atoms of bodies, it occurred to me that those vibratory motions of the æther which by their *attractive* effect maintain the regular gradation of density might be the primaries sought for; and this supposition is in accordance with the fact already adverted to, that magnetism pertains to the magnetized body apart from any extraneous action. [See, respecting "Attraction by Vibrations of the Air," an article in the *Philosophical Magazine* for April 1871. I cannot but regard the results of Mr. Guthrie's experiments as singularly confirmatory of my theoretical anticipations.] According to the views I have advocated relative to molecular forces, the maximum velocity of the attractive vibrations would be so much larger than that of the repulsive vibrations, that in the present inquiry the latter may be left out of account. Also it may be presumed that it is because that maximum velocity very much exceeds the rotatory, orbital, and translatory motions of the earth, that these motions have comparatively no magnetic effect.

10. Consequently, if, for simplicity, the magnet be supposed to be of a cylindrical form, in its interior an impulsive action upon the æther is continually operating in the directions parallel to its axis. Now as the *attractive* action of a series of undulations is in the direction *contrary* to that of propagation, and the attraction is towards the denser end of the magnet, it follows that the direction of the propagation, which is that of the maxi-

imum velocity in the condensed half of the wave, is towards the *rarer* end. At the same time, according to the mathematical result obtained in art. 8 of the article in the Philosophical Magazine for June 1872, the impulsive action on the æther, whether the primary vibratory motions be backwards or forwards, is towards the *denser* end, out of which consequently the generated streams flow.

11. The next point is to determine *the forms of the courses* of the magnetic streams generated under the above-described circumstances. To do this it is necessary to begin with admitting the truth of the following general hydrodynamical theorem, of which great use will be made in the subsequent investigations. [For proof of the theorem see art. 10 of the communication just cited.] It is not possible that the motion of an unlimited fluid mass can be such as to transfer any portion of the fluid on one side of an unlimited fixed plane to the other side without the transfer of an equal portion from the latter to the former. Thus the motion must be *circulating* or *reentering*; and accordingly a general characteristic of magnetic and galvanic currents is accounted for on the principles of hydrodynamics.

12. This being understood, the forms of the magnetic lines of motion are determinable, at least approximately, by the following argument. We have seen that in consequence of the regular gradation of the atomic density of a cylindrical magnet, and the velocities due to the outstanding undulations which by their attractive action maintain this state of density, the fluid is impelled in each transverse section at every instant towards the denser end of the magnet. These impulses operating against the inertia of the surrounding mass of fluid, have the effect of generating streams which, as being due to a steady action, are *steady*, and, as fulfilling the condition stated in art. 11, are necessarily *circulating*. To give a first idea of the courses of these streams, at least in the immediate neighbourhood of the magnet, I cannot do better than refer to the figure in p. 17 of the Astronomer Royal's 'Treatise on Magnetism,' the directions of the axes of the small magnets indicating (as will be shown subsequently) the directions of the lines of motion at the positions where they are situated. An approximate analytical expression for the forms of these magnetic curves is derivable from the present theory by the following investigation.

13. From what is argued above, the impulses are produced by variations of pressure due to variations of the square of the mean of the velocities within the cylinder estimated in directions parallel to its axis, *these* variations being caused exclusively by the mean contraction of channel resulting from the increasing

occupation of space by the atoms towards the denser end. Now we may conceive this mean effect to result from the separate effects of a vast number of atoms contained within a thin transverse slice of the cylinder, inasmuch as the individual motions due to the occupation of space by the atoms may coexist, and the parts of the motions resolved transversely to the axis will in that case destroy each other. Also it is to be considered that the motions of the æther resulting from the mean of the impulses must satisfy the condition of circulating.

14. This being understood, it will be seen to be allowable to substitute for the impulsive effect of contraction of channel that of a motion forward in the same direction of the aggregation of atoms contained in the above-mentioned slice, the fluid being relatively at rest. For on this supposition there will be a mean impulse parallel to the axis of the cylinder, which will be the sum of the impulses of the individual atoms resolved in that direction, and moreover *will give rise to a circulating motion*. The last assertion rests on Poisson's solution of the problem of the simultaneous motions of a ball-pendulum and the surrounding fluid, according to which the lines of motion of the fluid are *reentering*; and this being the case with respect to each atom, the result of the composition of all the motions will be circulating motion. Now, assuming the transverse section of the cylinder to be small, it is evident that the stream resulting from the action of all the atoms in the slice will have *quam proxime* the same form as that produced by a single atom situated at the middle point of the slice. But by Poisson's solution we obtain the analytical expression of the motion of the fluid in this case. Hence a formula for expressing the motion due to all the atoms in a given small slice may be at once inferred.

15. Let A and B be the extreme points of the axis of the cylinder, O its middle point, P any extraneous point the coordinates of which reckoned from O along and perpendicular to the axis are p and q , and Q being a point of the axis distant by x from O; let the straight line joining P and Q make an angle θ with the positive direction of the axis. Then if $PQ=r$, μ be the velocity of the atom, and α its radius, by the above-mentioned solution the velocity at P in the direction from Q to P is $\frac{\mu\alpha^3}{r^3} \cos \theta$, and that perpendicular to P Q tending in the negative direction is $\frac{\mu\alpha^3}{2r^3} \sin \theta$. Hence, denoting by X and Y the total velocities resolved along and transversely to the axis, we have

$$X = \frac{\mu\alpha^3}{r^3} \cos^2 \theta - \frac{\mu\alpha^3}{2r^3} \sin^2 \theta, \quad Y = \frac{\mu\alpha^3}{r^3} \cos \theta \sin \theta + \frac{\mu r^3}{2r^3} \sin \theta \cos \theta;$$

or, since $\cos \theta = \frac{p-x}{r}$, $\sin \theta = \frac{q}{r}$, and $r^2 = (p-x)^2 + q^2$,

$$X = \mu\alpha^3 \cdot \frac{(p-x)^2 + \frac{1}{2}q^2}{((p-x)^2 + q^2)^{\frac{5}{2}}}, \quad Y = \frac{3\mu\alpha^3 q(p-x)}{2((p-x)^2 + q^2)^{\frac{5}{2}}}.$$

Hence, to calculate the total velocity at P in the longitudinal and transverse directions, we have to add the velocities due to all the slices of given thickness dx from end to end of the magnet, or to obtain the integrals $k \int X dx$ and $k \int Y dx$ from $x = -l$ to $x = +l$, the length of the magnet being $2l$, and k a constant factor. The results will be found to be

$$\left. \begin{array}{l} \text{Longitudinal} \\ \text{velocity} \end{array} \right\} = \frac{k\mu\alpha^3}{2} \left\{ \frac{p-l}{((p-l)^2 + q^2)^{\frac{3}{2}}} - \frac{p+l}{((p+l)^2 + q^2)^{\frac{3}{2}}} \right\},$$

$$\left. \begin{array}{l} \text{Transverse} \\ \text{velocity} \end{array} \right\} = \frac{k\mu\alpha^3}{2} \left\{ \frac{q}{(q^2 + (p-l)^2)^{\frac{3}{2}}} - \frac{q}{(q^2 + (p+l)^2)^{\frac{3}{2}}} \right\}.$$

16. It will now be shown that these velocities are proportional to the *directive actions* of the magnet in the longitudinal and transverse directions on a small needle having its centre at P, and movable about an axis perpendicular to the plane containing P and the axis of the cylinder. The small magnet will be supposed to be surrounded by magnetic streams exactly like those which, according to the foregoing theory, belong to the large magnet, and to be of such small dimensions that the streams from the large magnet may be considered to have the same direction and velocity at all the positions of the atoms of the other. To find the action of the large magnet on the small one, it is now required to determine for any point the accelerative action of the pressure of the fluid resulting from the coexistence of the two sets of motion.

17. It is clearly not necessary to take account of any force acting perpendicularly to the plane passing through P and the axis of the cylinder, because all such forces are equal and opposite on the two sides of the plane. Let, therefore, that plane contain the axes of x and y , and let u_1, v_1 be velocities, parallel to the axes, due to the large magnet, and u_2, v_2 be those due to the small one. Then by hydrodynamics, the motion being steady, and, as vanishing at an infinite distance, such as makes $u dx + v dy + w dz$ an exact differential, we have

$$p = C - \frac{1}{2}((u_1 + u_2)^2 + (v_1 + v_2)^2).$$

Let V_1 be the velocity of the incident stream of the large magnet, and let its direction make an angle θ_1 with the axis of x . Then $u_1 = V_1 \cos \theta_1$ and $v_1 = V_1 \sin \theta_1$. Again, let the velocity

of the small magnet's stream at the position of any one of its atoms be V_2 , and be in a direction making the angle α with the axis of the magnet, and let this axis make the angle θ_2 with the axis of x . Then we have

$$u_2 = V_2 \cos (\theta_2 - \alpha), \quad v_2 = V_2 \sin (\theta_2 - \alpha).$$

Hence, by substituting in the above expression for p ,

$$p = C - \frac{1}{2} (V_1^2 + 2V_1V_2 \cos (\theta_1 - \theta_2 + \alpha) + V_2^2).$$

The pressure p , so far as it depends on the term V_2^2 , can have no effect in producing either rotation or motion of translation of the small magnet, because the velocities V_2 are symmetrical both with respect to its axis, and to the transverse plane passing through its centre. Hence, omitting this term, we have, since V_1 and θ_1 have been supposed constant, and θ_2 is a constant angle,

$$-\frac{dp}{dx} = V_1 \cos (\theta_1 - \theta_2) \frac{d \cdot V_2 \cos \alpha}{dx} - V_1 \sin (\theta_1 - \theta_2) \frac{d \cdot V_2 \sin \alpha}{dx};$$

so

$$-\frac{dp}{dy} = V_1 \cos (\theta_1 - \theta_2) \frac{d \cdot V_2 \cos \alpha}{dy} - V_1 \sin (\theta_1 - \theta_2) \frac{d \cdot V_2 \sin \alpha}{dy}.$$

18. We may now simplify the reasoning, without loss of generality, by supposing that the axis of x coincides with the axis of the small magnet, or that $\theta_2 = 0$. In that case $\Sigma \cdot -\frac{dp}{dx} = 0$, because by reason of the symmetry of the motion the positive values of $\frac{d \cdot V_2 \cos \alpha}{dx}$ are just counteracted by the negative, and the same is the case with respect to the values of $\frac{d \cdot V_2 \sin \alpha}{dx}$.

Hence the forces parallel to the axis of the magnet have no tendency to produce motion of translation. Neither do they tend to produce rotation, because corresponding to a force at any point on one side of the axis there is an equal force at an equal distance on the other side, and equally distant from the axis of motion. We have thus only to consider the effects of the forces $-\frac{dp}{dy}$. Now the sum of these forces is zero, because by reason of the symmetry of the motion, the sums of the positive values of $\frac{d \cdot V_2 \cos \alpha}{dy}$ and $\frac{d \cdot V_2 \sin \alpha}{dy}$ are respectively equal to the sums of their negative values. Hence there is no tendency to motion of translation transversely to the axis. Also the forces expressed

by the different values of the first term of the formula for $-\frac{dp}{dy}$ produce no motion of rotation, because they are equal and in the same direction at equal distances from the axis of rotation on the *same* side, whether positive or negative, of the axis of the magnet.

19. But the forces expressed by the second term of the same formula are at a given distance from the axis of motion equal and in the *same* direction at points equally distant from the axis of the magnet on *opposite* sides, and at the same time the directions are *opposite* on the opposite sides of the axis of motion. Accordingly these forces produce motion of rotation, and are the only forces that have this effect. Hence if x_1 be the distance of any atom from the axis of rotation, the whole momentum of rotation is proportional to

$$-V_1 \sin \theta_1 \times 2 \Sigma . x_1 \frac{d . V_2 \sin \alpha}{dy},$$

the summation embracing all the atoms on *one* side of the axis of rotation. It is now to be considered that the accelerative action of the fluid in *steady* motion on any atom in any direction has a constant ratio to the accelerative force in the same direction of the fluid itself at the position of the atom. [This proposition is proved in pp. 313-315 of 'The Principles of Mathematics and Physics.'] Hence, if H be a constant factor having a certain ratio to the result of the above summation, the directive force of the incident current will be

$$HV_1 \sin \theta_1,$$

tending always to place the axis of the small magnet in such a position that its proper current along the axis and the incident current flow in the *same* direction, in which case $\theta_1=0$.

20. It follows from the foregoing argument that the longitudinal and transversal components of a stream from a large magnet incident upon a small one are proportional to the directive forces of the stream in the two directions, and that consequently the forces may be supposed to be expressed by the formulæ for the velocities obtained in art. 15.

I take occasion here to remark that the Astronomer Royal has deduced in the Philosophical Transactions (vol. clxii. p. 492) expressions for the same forces wholly different from those in art. 15, by assuming the intensity of the magnetism along the axis of a magnet to vary proportionally to the distance from its centre, and finds that they give numerical results which do not sufficiently agree with experiment. According to the theory I am advocating that assumption is not allowable.

21. I proceed now to account by the hydrodynamical theory for the experimental facts on which the Gaussian argument for the law of the inverse square in magnetism rests. In one set of experiments the small magnet was placed so that the prolongation of the axis of the large magnet passed through its centre and cut its axis at right angles. Under these circumstances the ordinate $q=0$, so that the transverse velocity vanishes, and the expression for the longitudinal velocity becomes

$$\frac{k\mu\alpha^3}{2} \left(\frac{1}{(p-l)^2} - \frac{1}{(p+l)^2} \right),$$

which, if the ratio of l to p be small, is very nearly

$$\frac{2k\mu\alpha^3 l}{p^3}.$$

In another set of experiments the small magnet was placed with its axis pointing to the centre of the large one transversely to the axis; in which case, since $p=0$, the transverse velocity again vanishes, and, supposing the ratio of l to q to be small, the approximate expression for the longitudinal velocity becomes

$$-\frac{k\mu\alpha^3 l}{q^3}.$$

Hence in both cases the directive force varies inversely as the *cube* of the distance from the centre of the large magnet, and at equal distances is *double* in the former case to what it is in the other. The two principal results of the experiments having been thus accounted for, the hydrodynamical theory has effected, at least to a first approximation, all that may strictly be demanded from it. In order, however, to exhibit its applicability more fully, I shall now employ it to show why Gauss's empirical theory succeeds in representing the same facts.

22. It has been inferred from the hydrodynamical theory that the action of the large magnet on the small one is simply *directive*. Hence, assuming that each magnet has near its ends a positive pole and a negative pole, and that like poles repel each other and unlike poles mutually attract, it will readily be seen that, according to the arrangements of the two experiments described in art. 21, the actions on the poles of the small magnet are nearly equal, and nearly in the same direction, and that the action on one is attractive and that on the other repulsive. These forces are, therefore, proper for acting as a kind of *couple*, and giving direction to the axis of the needle. Also in this mode of viewing magnetic action, if, as is empirically assumed, the force varies inversely as some power of the distance from the pole, the law of the inverse *square* is alone applicable, because experiment and the hydrodynamical theory concur in indicating that the direc-

tive force varies nearly as the inverse *cube* of the distance from the centre of the magnet, which law results from the joint action of an attractive and a repulsive force expressed thus,

$$\pm \left(\frac{\mu}{r^2} - \frac{\mu}{(r + \delta r)^2} \right),$$

the differential force being nearly $\frac{2\mu\delta r}{r^3}$, which for a given value of δr varies inversely as the cube of the distance.

23. The whole preceding argument points to the conclusion that the assumed attractive and repulsive magnetic forces have only a hypothetical existence, and that what really exists is hydrodynamical pressure.

24. Proceeding now to discuss in a similar manner the problem of the action of a galvanic coil on a small magnet, I propose, first, to solve it according to the principles of the hydrodynamical theory of galvanism, and then to inquire how far the same theory will account for the facts and hypotheses on which Ampère's empirical solution of the problem rests. The hydrodynamical considerations will differ in some essential respects from those applicable to magnetism.

25. First it will be necessary to ascertain what motions of the æther correspond to the transmission of a galvanic current along a fine wire. For this purpose certain hydrodynamical theorems will be employed, the principles and the proofs of which I have discussed in various antecedent researches. I consider it to be an axiom that, whatever be the motion of a fluid mass, the lines of direction of the motion may at all times be cut by a surface made up of portions, either finite or indefinitely small, of different surfaces of continuous curvature, so joined together that the tangent planes at the points of junction of two contiguous portions do not make a *finite* angle with each other. The reason for the latter condition is a *dynamical* one, whereby infinite forces are excluded. The other is an abstract geometrical condition of continuity, to which the directions of the motion of a fluid assumed to be *continuous* are necessarily subject, and in virtue of which the motion admits of being *calculated*. If any one thinks that there are motions of a fluid which this condition does not embrace, let him calculate them if he can; I do not concern myself with them.

26. It follows from the foregoing theorem that the general differential equation of the above-defined surfaces of displacement is (according to the usual notation) $udx + vdy + wdz = 0$, and that consequently the left-hand side of this equality is either integrable of itself or by a factor. Reasoning on the principle that this must be the case always and at all points of the fluid,

I have obtained a general hydrodynamical equation in which the factor enters as an unknown quantity. The present investigation does not require reference to that equation further than to state that it serves to demonstrate the reality of the factor, and consequently to establish the truth of the equation

$$u\left(\frac{dv}{dz} - \frac{dw}{dy}\right) + v\left(\frac{dw}{dx} - \frac{du}{dz}\right) + w\left(\frac{du}{dy} - \frac{dv}{dx}\right) = 0, \quad (a)$$

which, as is known, is the general expression of the condition that $u dx + v dy + w dz$ is integrable by a factor.

27. I have recently learnt with some surprise from more than one quarter that the equation (a), and, by consequence, the antecedent views on which it is founded, are considered to be untrue for reasons drawn from a discussion on certain hydrodynamical questions which I had with Professor Stokes in the *Philosophical Magazine* so long ago as 1842. Claiming to adopt views expressed by Professor Stokes on that occasion, a correspondent sends me the following argument relative to the equation (a). Conceive to be impressed on all parts of the fluid the arbitrary constant velocities α, β, γ in the directions of the axes of coordinates. Then the equation becomes

$$(u + \alpha)\left(\frac{dv}{dz} - \frac{dw}{dy}\right) + (v + \beta)\left(\frac{dw}{dx} - \frac{du}{dz}\right) + (w + \gamma)\left(\frac{du}{dy} - \frac{dv}{dx}\right) = 0,$$

which, since α, β, γ are perfectly arbitrary, cannot be true unless

$$\frac{dv}{dz} - \frac{dw}{dy} = 0, \quad \frac{dw}{dx} - \frac{du}{dz} = 0, \quad \frac{du}{dy} - \frac{dv}{dx} = 0;$$

that is, unless in every instance of the motion of a fluid $u dx + v dy + w dz$ is an exact differential. As this is certainly not the case, it is concluded that the equation (a) is untrue.

28. The answer to this argument is that the equation (a) was deduced on the principle of its being exclusively applicable to motions which are peculiar to a fluid, and which, consequently, a solid is not capable of, the motions, namely, by which the parts of a fluid mass in motion can change their relative positions. This is the sole *raison d'être* of the equation. Hence the introduction of the velocities α, β, γ common to all the parts of the fluid is a violation of the principle on which it is founded; or rather the above argument is a proof *à posteriori* that the equation *excludes* such common velocities. If, therefore, that equation be satisfied, there is no need to "define" the velocity that may be common to all the parts of the fluid; for either such motion takes place under given conditions, and is consequently known, or, if not known and not knowable (whether it be due to the earth's rotatory and orbital motions, or to the motion of the

solar system in space), the determination of the motions whereby the individual parts of the fluid alter their relative positions remains the same.

29. If while such change of relative position is taking place each rectangular fluid element is also changing form, the lines of motion are necessarily not parallel; and since, by hypothesis, they are in the directions of normals to continuous curved surfaces, it follows that for such motions $udx + vdy + wdz$ is an exact differential. But if each rectangular element retains the same form, the lines of motion must be parallel, the surfaces of displacement are planes, and $udx + vdy + wdz$ is integrable by a factor.

In the course of my many hydrodynamical researches I have had from time to time the benefit of criticisms, and arguments *ex adverso*, from my mathematical contemporaries; and I willingly admit that I have thereby been induced in several instances to modify my original views. But hitherto I have not perceived that there is any ground for questioning the truth of the principles and the reasoning which have conducted me to the equation which I call the third general equation of hydrodynamics, and I have consequently not hesitated to employ the equation (a), which is a logical consequence of that general equation, in laying a foundation for the subjoined hydrodynamical theory of the action of a galvanic coil.

30. A current of the æther being supposed to flow uniformly along a straight cylindrical conductor, the motion of the fluid at any point may be determined by the following reasoning (given in more detail in the 'Principles of Physics,' pp. 563-565). The motion is plainly a function of the distance from the axis of the cylinder, but cannot be wholly parallel to it; for if that were the case, since the motion is, by hypothesis, steady, and in such motion the pressure is everywhere less as the velocity is greater, and since in this instance the velocity will evidently be less the greater the distance from the axis, it would follow that on all sides there would be tendency to motion towards the axis, which, if not counteracted, would put a stop to the current. To counteract this tendency there must be centrifugal force due to circular motion about the axis; and according to the hydrodynamics of steady motion the rectilinear and circular motions may coexist. Hence, if r be the distance from the axis, and the rectilinear and circular motions at that distance be respectively $F(r)$ and $f(r)$, we shall have

$$u = \frac{y}{r} f(r), \quad v = -\frac{x}{r} f(r), \quad w = F(r).$$

These equations satisfy the condition of constancy of mass ex-
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pressed by the equation

$$\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0,$$

which is true for a *compressible* fluid inclusively of small terms of the *second* order; so that the subsequent reasoning, although strictly applicable to an incompressible fluid, may be taken to apply to the æther. Now, from the known expressions for $\frac{dp}{dx}$, $\frac{dp}{dy}$, $\frac{dp}{dz}$ for steady motions of an incompressible fluid, it will readily be found that

$$\frac{(dp)}{dr} = \frac{(f(r))^2}{r} = \text{the centrifugal force.}$$

31. These results are independent of the forms of the functions $f(r)$ and $F(r)$ and of any relation between them. But since the assumed values of u , v , w do not make $u dx + v dy + w dz$ an exact differential, according to the principles maintained above, they must be such as to satisfy the equation (a). By substituting them in that equation, and integrating, the result is

$$\frac{f(r)}{F(r)} = \frac{c}{r},$$

c being the arbitrary constant introduced by the integration. We have thus demonstrated that the current must be such as to satisfy the relation between the velocities $f(r)$ and $F(r)$ indicated by this equation.

32. By taking account of this relation the equation

$$u dx + v dy + w dz = 0$$

gives

$$dz = - \frac{f(r)}{r F(r)} (y dx - x dy) = \frac{c}{r^2} (x dy - y dx).$$

Hence, by integration,

$$z = c \tan^{-1} \frac{y}{x} + b,$$

which is the general equation of the surfaces of displacement, the orthogonal trajectories of which determine the directions of the motion. If $\tan^{-1} \frac{y}{x} = \theta$, and r_1 be a given distance from the axis, we have

$$\frac{r_1 \theta}{z - b} = \frac{r_1}{c};$$

which shows that the motion in the cylindrical surface of radius r_1 consists of spiral motions the directions of which make with

parallels to the axis the angle whose tangent is $\frac{r_1}{c}$. The motion is thus completely determined if only the forms of the functions $f(r)$ and $F(r)$ can be found. In my work already cited I have expressed (in p. 566) a doubt as to the practicability of doing this in the existing state of hydrodynamics. I have, however, since discovered the following argument, which I consider to be adequate to this purpose.

33. Suppose the straight galvanic current to be cut by a plane transversely, and on the plane three concentric circles to be described having the common centre on the axis, and let their radii be $r + \alpha$, r , and $r - \alpha$, α being very small. Also let there be drawn in the plane from that centre two straight lines separated by the small angle $\delta\theta$. Then the space bounded by these lines

and the first and second circles is $\left((r + \alpha)^2 - r^2\right) \frac{\delta\theta}{2}$, and that bounded by the same lines and the second and third circles is $\left(r^2 - (r - \alpha)^2\right) \frac{\delta\theta}{2}$. Now, according to the foregoing investigation,

these spaces may be considered to be transverse sections of elementary *channels* in which the galvanic current is *constrained* to move. Let V' be the mean velocity of the current through the space furthest from the axis, and V that of the current through the other. Then, inclusively of small terms of the second order,

$V' = F\left(r + \frac{\alpha}{2}\right)$, and $V = F\left(r - \frac{\alpha}{2}\right)$. Hence the excess of fluid which in a second of time passes through the larger space above that which in the same time passes through the other is

$$F\left(r + \frac{\alpha}{2}\right)(2r\alpha + \alpha^2) \frac{\delta\theta}{2} - F\left(r - \frac{\alpha}{2}\right)(2r\alpha - \alpha^2) \frac{\delta\theta}{2},$$

which, omitting terms containing α^3 &c., becomes

$$\alpha \left(\frac{F(r)}{r} + \frac{d \cdot F(r)}{dr} \right).$$

34. We have now to take into account the principle adverted to in art. 11, according to which the inertia of an unlimited mass of incompressible fluid opposes an insuperable obstacle to any alteration of the quantities of the mass on the two sides of any unlimited fixed plane. Since in the case of the galvanic current fluid is being transferred every instant across the above-mentioned transverse plane, not only must the rheophore furnish a channel for the *circulation* of the fluid, but there must also be a general *stress*, like hydrostatic pressure, which, taking effect always in the directions of any channels of circulation, maintains

the current in opposition to the tendency of the inertia of the fluid to put a stop to it. In the present theory this stress is due to the action of the battery; the wire supplies a channel for the current; and, as is shown in art. 11, it is dynamically necessary that the current should flow in a complete circuit.

35. It is clearly possible that the form of the function $F(r)$ might be determined by arbitrary conditions. For instance, if the above-mentioned stress were arbitrarily caused to be the same at all points of the transverse plane, the velocity parallel to the axis would be the same at all points, and $F(r)$ would be a constant. But it is evident that this is not true of a galvanic current. The principle of the present inquiry demands that as a definite relation between the functions $f(r)$ and $F(r)$ was obtained in a unique manner by integration, the form of the function $F(r)$ should be similarly determined. Now the only way in which that form can be obtained exclusively by integration is to equate to zero the above factor $\frac{F(r)}{r} + \frac{d \cdot F(r)}{dr}$, in which case integration gives

$$F(r) = \frac{c}{r}.$$

Thus the velocity parallel to the axis varies inversely as the distance from the axis; and the stress which maintains that velocity, and is therefore proportional to it, varies according to the same law. Since the transverse sections of the elementary channels above defined vary *directly* as the distances, it follows that through each elementary channel outside the wire the same quantity of fluid flows in a given interval. Also, since it has been shown (art. 31) that $f(r) = \frac{c}{r} F(r)$, we obtain

$$f(r) = \frac{c_1}{r^2},$$

or the transverse circular motion varies inversely as the *square* of the distance. These results are essential to the hydrodynamical theory of galvanism.

36. But for the theory of the action of a galvanic coil we require to know the motion of an ætherial current along a fine wire the axis of which has the form of a circle, and the transverse section of which is circular and uniform. For this case it will be assumed that, by reason of symmetry, the motion at any given point is compounded of motion parallel to the axis, and of motion in the plane passing through the point and the centre of the axis, and cutting the axis at right angles. Let the plane of this axis be parallel to that of xy , and its centre be on the axis of z ; and let h be the height above the plane xy of the point of intersection of the circular axis by the above-mentioned trans-

verse plane, and a the distance of the same point from the axis of z . Also in the same plane let r and θ be the polar coordinates of the given point P referred to the point of intersection as pole, and to the straight line through the pole parallel to the plane xy . Then, the rectangular coordinates of P being x, y, z , if we put R for $(x^2 + y^2)^{\frac{1}{2}}$, and suppose the velocity parallel to the plane xy to be $F(R, z)$, and that in the transverse plane to be $f(r, \theta)$, we have

$$\begin{aligned} u &= f(r, \theta) \frac{z-h}{r} \cdot \frac{x}{R} + F(R, z) \frac{y}{R}, \\ v &= f(r, \theta) \frac{z-h}{r} \cdot \frac{y}{R} - F(R, z) \frac{x}{R}, \\ w &= -f(r, \theta) \frac{R-a}{r}, \end{aligned}$$

together with the equalities $r^2 = (z-h)^2 + (R-a)^2$, and $\tan \theta = \frac{z-h}{R-a}$. By analytical operations, the details of which, as being somewhat long but presenting no difficulties, are not inserted here, it may be shown (1) that $\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0$; (2) that $u dx + v dy + w dz$ is *not* an exact differential; (3) that by substitution in the equation (a) there results the following equation of condition connecting the functions f and F :

$$r \frac{df}{dr} + \frac{a}{R} - \frac{dF}{F dz} (z-h) - (R-a) \frac{dF}{F dR} = 0. \quad (b)$$

37. Respecting this equation we may, first, remark that since it does not contain $\frac{df}{d\theta}$, it shows that the assumed motion requires that f should be a function of r only, and consequently the motion in planes transverse to the axis of the wire is proved to be circular. This result is in accordance with the original assumption, that the transverse section of the wire is circular, as should plainly be the case, since the surface of the wire bounds the circular motion.

38. The proof that f is a function of r only having taken no account of the magnitude of a , and being clearly independent of that of h , we may infer that the function has the same form whatever be the radius of the axis of the wire, and therefore the same as if the radius were infinite, in which case a finite portion of the wire might be considered to be a straight cylinder. But we have shown (art. 35) that for the straight cylinder $f(r) = \frac{c_1}{r^2}$. Consequently, substituting this value of $f(r)$ in the equation (b),

there results for determining the form of the function F the equation

$$-2 + \frac{a}{R} - \frac{dF}{FdZ}(z-h) - \frac{dF}{FdR}(R-a) = 0.$$

This partial differential equation integrated in the usual way gives

$$F = \frac{1}{R(z-h)} \cdot \phi\left(\frac{R-a}{z-h}\right).$$

Now it is certain that the expression for the velocity $F(R, z)$ must involve the distance r of the point P from the axis of the wire. This condition is satisfied by the above value of F by assuming that

$$\phi\left(\frac{R-a}{z-h}\right) = \left(1 + \frac{(R-a)^2}{(z-h)^2}\right)^{-\frac{1}{2}},$$

and can be satisfied in no other way. We have therefore the unique solution

$$F = \frac{C_2}{R((z-h)^2 + (R-a)^2)^{\frac{1}{2}}} = \frac{C_2}{Rr},$$

C_2 being an arbitrary constant. Thus exact expressions for the velocity in any plane transverse to the axis and for that parallel to the axis having been found, the total motion, which is composed of these two, is completely determined.

39. From the above expression for F , that which applies to a straight cylindrical wire may readily be deduced. For putting $a + \alpha$ for R , α being a variable quantity restricted within comparatively small limits, and giving to C_2 the form $c'a$, c' being an arbitrary factor, we have

$$F = \frac{c'a}{(a+\alpha)r} = \frac{c'}{\left(1 + \frac{\alpha}{a}\right)r},$$

which for a straight wire, for which a is infinite, becomes $\frac{c'}{r}$, agreeing with the result obtained in art. 35.

40. It would seem that the foregoing investigation might be generalized so as to apply to a wire conductor of any form, when it is considered that the determinations in arts. 30–35 of the forms of $F(r)$ and $f(r)$ for a straight cylinder did not involve the length of the axis, and would remain the same for a cylinder of infinitesimal length if the condition of circular motion about the axis were satisfied. We have shown that this condition is in fact satisfied by a uniform conductor of circular form, which may be regarded as made up of a series of right cylinders of infinitesimal

lengths; and as any portion of a uniform conductor of any form may be supposed to be similarly composed, the expressions for $f(r)$ and $F(r)$ for a circular wire would appear to apply generally, if the radius a be taken to represent the varying radius of curvature of the axis of the wire. This question, however, requires more consideration than I can now give to it.

41. Returning now to the circular conductor, if in the expressions for u, v, w in art. 36 the values found for $f(r, \theta)$ and $F(R, z)$ be substituted, we shall have

$$\begin{aligned} u &= \frac{c_1 x(z-h)}{Rr^3} + \frac{c_2 y}{R^2 r}, \\ v &= \frac{c_1 y(z-h)}{Rr^3} - \frac{c_2 x}{R^2 r}, \\ w &= -\frac{c_1(R-a)}{r^3}. \end{aligned}$$

Hence, since $R^2 = x^2 + y^2$ and $r^2 = (z-h)^2 + (R-a)^2$, it will be found that

$$u dx + v dy + w dz = \frac{c_1}{r} \cdot \frac{(z-h)dR - (R-a)dz}{(z-h)^2 + (R-a)^2} + \frac{c_2}{r} \cdot \frac{ydx - xdy}{x^2 + y^2}.$$

Consequently the right-hand side of this equation becomes an exact differential when multiplied by the factor r . Before proceeding to the next step, it is necessary to take into account that in the foregoing investigation the arbitrary constants c_1 and c_2 have been introduced in such manner as to show that they are *wholly independent of each other*. Hence, on equating $r(udx + vdy + wdz)$ to zero, we must have separately

$$c_1 \frac{(z-h)dR - (R-a)dz}{(z-h)^2 + (R-a)^2} = 0, \quad c_2 \cdot \frac{ydx - xdy}{x^2 + y^2} = 0;$$

which means that both the motion transverse to the axis of the wire and that parallel to the same are such as require a factor for making $udx + vdy + wdz$ integrable. Both are steady motions and therefore coexist. Instead of the above two equations we may, by introducing an arbitrary constant factor λ , employ the single equation

$$c_1 \frac{(z-h)dR - (R-a)dz}{(z-h)^2 + (R-a)^2} + \lambda c_2 \cdot \frac{ydx - xdy}{y^2 + x^2} = 0.$$

Hence, by integration,

$$C = c_1 \tan^{-1} \frac{z-h}{R-a} + \lambda c_2 \tan^{-1} \frac{y}{x} = c_1 \theta + \lambda c_2 \phi,$$

supposing that $\frac{y}{x} = \tan \phi$, and, as before, that $\frac{z-h}{R-a} = \tan \theta$.

Differentiating this last equation, we get $0 = c \, d\theta + \lambda c_2 d\phi$, or $\frac{d\theta}{d\phi} = -\frac{\lambda c_2}{c_1}$.

42. Assuming that c_1 and c_2 are positive quantities in the expressions for u , v , w in art. 41, it will be seen that those values were formed so that θ and ϕ each *decrease* with the motion.

Hence $\frac{d\theta}{d\phi}$ will be positive, and λ must be a negative quantity. According to the supposed directions of the decrements, the spiral motion will be *dextrorsum*. If the motion were assumed to be such that $d\theta$ and $d\phi$ had different signs, the spiral motion would be *sinistrorsum*, and we might by the same reasoning as before obtain $C = c_1\theta + \lambda'c_2\phi$, c_1 and c_2 being still positive. In that case $\frac{d\theta}{d\phi} = -\frac{\lambda'c_2}{c_1}$ = a negative quantity, and λ' is consequently positive. As the factors λ and λ' are wholly arbitrary, we have thus shown that, as far as hydrodynamics is concerned, the galvanic current might be either *dextrorsum* or *sinistrorsum*.

Having in the preliminary part of this communication discussed the action of a large magnet on a small one, and having now ascertained the exact form of a galvanic current along a circular wire, I propose in a second Part to investigate the action of a galvanic coil on a small magnet, and to show why it agrees approximately with that of a magnet, and in what respect especially the two actions differ. In the course of the investigation the facts on which Ampère's theory rests will be accounted for by the hydrodynamical theory, for the purpose of fully establishing the claim of the latter to be considered a strictly *à priori* theory.

Cambridge, August 10, 1874.

XXX. On the Magnetization-Functions of various Iron Bodies.

By PROFESSOR A. STOLETOW*.

IN my work on the magnetization of iron† I have taken Neumann's coefficient κ as a measure of the magnetizability. This, as is well known, expresses the ratio in which the magnetic moment, referred to the unit of volume, stands to the quantity of the magnetizing force, presupposing that the iron forms an

* Translated from a separate impression communicated by the Author, from the *Bulletin de la Société Imp. des Naturalistes de Moscou*, 1873, No. 4.

† Pogg. *Ann.* vol. cxliv. p. 439; Phil. Mag. S. 4. vol. xlv. p. 40; more fully as a separate brochure in Russian, Moscow, 1872.

infinitely long cylinder and is uniformly magnetized longitudinally. I have named this coefficient (in that essay denoted by k) the *magnetizing-function* of the given iron, since it depends on the quantity of the magnetizing force. An analysis, namely, of the experiments of Quintus Icilius (with ellipsoids) and of my own (with a ring) showed that the function k at first increases rapidly as the decomposing force rises, and then again diminishes. This behaviour seems to take place with all sorts of iron; yet the absolute numerical values of k , with the same value of the argument, are very different, according to the quality of the iron. These results have been corroborated by a thorough investigation by Mr. H. A. Rowland*. He shows that the course of the function k is precisely similar for steel and nickel as well as iron, and can be represented by the same empiric formula, but that the constants of the formula, even for two varieties of one and the same metal, come out very different †.

Professor Riecke, in his "Contributions to the Knowledge of the Magnetization of Soft Iron" (Pogg. Ann. vol. cxlix. p. 433), proposes, instead of the magnetizing-function of the infinite cylinder, to consider another function p , which has the same signification in reference to the *sphere*.

The two quantities, referred to the same decomposing force,

* "On Magnetic Permeability, and the Maximum of Magnetism of Iron, Steel, and Nickel," Phil. Mag. August 1873, p. 140. The term "magnetic permeability" is used, after Sir W. Thomson, to denote the quantity $\mu = 1 + 4\pi k$, which, as k is here generally much greater than unity, varies nearly proportionally with k .

† Professor Wiedemann, when discussing my work (in *Galvanismus*, 2nd ed. vol. ii. p. 518), regards the function which is calculated from experiments with the ring as *another* magnetization-function, not to be confounded with that obtained from experiments with "unclosed systems." There does not seem to me sufficient reason for this distinction. Residual magnetism, which is here in question, is present in bars also. If we consider a very thin and long bar and a ring, both magnetized *uniformly*, the difference between them in relation to the residual magnetism is hardly to be reckoned considerable. The demagnetizing force proceeding from the mass of its iron will in the ring be equal to *nil*; in the bar it is a small quantity, of the order of $\frac{\omega}{l^2}$, where ω is the cross section, and l the length

of the bar. (Maxwell, 'Treatise on Electricity and Magnetism,' vol. ii. p. 67.) In both cases an external force is requisite in order to expel the residual magnetism. If we always observe the *reversal of the magnetism* of the iron, the calculation of k is only to this extent vitiated by the residual magnetism, that a certain portion of the reversed decomposing force is expended in discharging it. But M. Wiedemann's own experiments with bars, and those of Poggendorff with closed systems (Wiedemann, *l. c.* p. 519), show that that portion is only very little.

A survey of the numbers obtained by Mr. Rowland, partly with bars, partly with rings, establishes that the most essential cause of their difference is not the *form*, but the *quality* of the material.

are connected by the relation

$$p = \frac{1}{\frac{4\pi}{3} + \frac{1}{k}}.$$

The function p , he says, deserves the preference because, "within a very large sphere of magnetizing forces, it possesses a nearly constant value for all sorts of iron" (*l. c.* p. 435). The values of p calculated by M. Riecke from his own and others' experiments do in fact accord very well; they give (p. 470) as mean value for moderate decomposing forces the number 0.2372, and as maximum value

$$p = 0.2382.$$

The purpose of the present note is to bring out that these results are self-evident, and the above numbers have a very simple meaning; they are, namely, pretty close approximations to the number

$$\frac{3}{4\pi} = 0.2387,$$

which is obtained as the upper limit of p when we put $k = \infty$, and consequently represents the *ideal maximum* of p . With moderate decomposing forces $\frac{1}{k}$ is always small compared with $\frac{4\pi}{3}$ (since k here lies somewhere between 20 and 200*), and may, in the first approximation, be neglected. On this account p remains always nearly constant and *independent of the quality of the iron*†. Indeed, for every other strongly magnetic material, about the same value of p would result‡.

From this we see, on the one hand, that the numbers calculated by M. Riecke furnish a fair confirmation of the theoretical consideration; but, at the same time, we see that the quantity p is very little suitable for characterizing the magnetizability of a material, since for the sphere the influence of the quality of the substance nearly vanishes before the influence of the form. It can be proved that this holds good generally for every body the

* For my iron ring the maximum of k was = 174; with the kinds of iron investigated by Mr. Rowland it was in nearly every case higher, and in one case reached the value $k=439$ ($\mu=5515$).

† A brief note in reference to this I find in Wiedemann's *Galvanismus*, 2nd ed. vol. ii. p. 403.

‡ For a ring of annealed nickel Mr. Rowland found the maximum of $k=24$ ($\mu=305$). According to this, even for nickel (at its maximum of magnetizability) p may reach the value 0.2364. For steel the approximation to the absolute maximum 0.2387 becomes still closer, and holds between wider limits of the decomposing force.

dimensions of which in all directions are of the same order*. In order to calculate *à priori*, with satisfactory accuracy, the magnetization of bodies so formed, a rough estimation of the coefficient k is sufficient. The magnetization-functions of such bodies, ascertained by experiments, will always exhibit much less variability than that of a thin bar or ring, of a thin plate or scale, and may almost be regarded as constant. But if, starting from such mean value, we try to calculate the magnetization of any body of the category last mentioned, we may arrive at very inaccurate results; for, with bodies *one or two dimensions of which are very small in comparison with the third, the tangential component* of the magnetic moment will, with the same decomposing force, increase proportionally with k †. The influence of the specific qualities of the substance appears here, therefore, in full intensity. If we wish to bring such bodies also within the range of our considerations, we must take into account the specific quality of the substance, and the knowledge of the magnetization-functions of bodies of this sort will be indispensable. The function k perfectly suffices for this purpose, and has the advantage that in it abstraction is made of the transverse dimensions of the thin body.

Those bodies the dimensions of which are of different orders of magnitude play a peculiar part in several branches of physics. In hydrostatics their theory is most essentially conditioned by the capillary forces. In the science of elasticity they require a special method of treatment; in that of paramagnetic magnetization they make a very precise knowledge of the magnetization-functions absolutely indispensable.

Christmas (O. S.) 1873.

* Compare pp. 56–67, vol. ii. of Maxwell's Treatise—for example, "When κ is a large positive quantity, the magnetization depends principally on the form of the body, and is almost independent of the precise value of κ , except in the case of a longitudinal force acting on an ovoid so elongated," &c. (p. 66). We always presuppose here that the magnetization is *uniform*.

† More strictly, proportionally with $\frac{k}{1+k\epsilon}$, where ϵ is a number vanishing with the transverse dimensions, and the value of k is not referred to the whole tangential force of decomposition T , but to $\frac{T}{1+k\epsilon}$. For a limited bar $\epsilon=0$. These considerations explain, for example, the experiments of Von Waltenhofen on the magnetization of bundles of thin wires, thin-walled tubes, &c. (Wiedemann's *Galvanismus*, 2nd ed. vol. ii. p. 430). The great power of the magnets composed of thin bands of steel (*rubans d'acier*) of M. Jamin (*Comptes Rendus*, vol. lxxvi. p. 789) appears also to stand in relation therewith (compare especially art. X. p. 794).

XXXI. *On Tides and Waves.—Deflection Theory.*

By ALFRED TYLOR, F.G.S.

[With Three Plates.]

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

London, August 15th, 1874.

I SHOULD be glad if any of your readers will send me a reference to any work of authority in which there is any direct statement of the height of the level of the ocean (say the central Atlantic) compared with high-water mark on the east and west coasts of Ireland and England. This is an important point in the general theory of the tides, a subject I am about to discuss. The view I shall advocate is that the level of the ocean is nearly represented by high-water mark on coasts and bays where there is free access of the tide and a channel without a sudden taper. Mr. E. Roberts, of the Nautical Almanac Office, editor of the Reports of the Tidal Committee of the British Association, informed me last month he was not aware of any statement in print on good authority on this point. The only opinion I have on this subject is from Professor G. G. Stokes, F.R.S. (and that is an unprinted one*), who wrote, "Nobody maintains that the general level of the ocean is that of low water; it is the mean between high and low, except in shallow channels &c., where it is not the exact mean." In the absence of further authorities I shall give my deflection theory of tides.

In Plates II. and III. I give a drawing of what I suppose is the relation of high and low water on the coast and in estuaries and channels to that of the sea. I show that the level of the central ocean approximates to mean high-water mark on the coast of Ireland, and is about 4 feet above the English Ordnance Datum, which datum may be treated as an arbitrary line, being only the mean level of the sea at Liverpool, Penzance, and Falmouth, all places in which the tide is affected by the converging contour of the coast.

The velocity of the central ocean-stream, if reduced by the inequalities of the sea-bottom at a different ratio to the depth, would cause the water to heap, and *vice versâ*. I do not think it does heap perceptibly until near the coast, and then in very different degrees (see Plates II. and III.). When the increase of velocity exactly balances the decrease of depth; that is, using V and v for the old and new velocity, and l and i for the respective distances from the centre of the earth,

$$\text{when } l = i, \text{ then } \frac{V}{v} = \frac{\rho}{\rho'} \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

* In some remarks about the views expressed in Plates II. and III. sent to him for examination, March 7, 1874.

This is the equation to equilibrium of the ocean-surface in the case where no interfering currents, caused by difference of temperature in the ocean, are present*. A complete oceanic tide stretches from coast to coast, and is always divided into three regions (central, anterior flowing, and posterior ebbing), reversing direction each six hours; that is, in the part where there was propulsion, aspiration succeeds, and *vice versâ*. A perfect tide would stretch over a space on parallel of latitude represented by the rotation of the earth in six hours.

The mass of the central ocean is represented, in Pl. II. figs. 1 and 2, and Pl. III. fig. 1, as moving 180 feet per hour on the average of each tide of six hours, but in alternate and opposite directions. A movement of 3 feet per minute in the central ocean 20,000 feet deep would communicate a velocity of three miles an hour where the water was 238 feet deep, by the composition of forces. I suppose that this slow motion in a vast mass of water of great and equal depth would be horizontal alone, as it is not possible to suppose vertical motion without creating a gap below or behind the tidal current. The horizontal motion would be also limited; for the sum of the motion of all the particles of water in the Atlantic tidal stream could not exceed the area of the gap emptied and filled on the opposite coasts of the Atlantic each alternate tide. In this respect the tide is like a wave, the relation of whose movements to the size of the gap made when generated is clearly shown in fig. 1 (p. 216). The force of the moon will be estimated; and the relation of its attraction to a particle on the ocean is shown in fig. 4, Pl. IV. The direction in which the moon can affect

* From the equation $Q = AV$, using Q and q for discharge per second, and A for cross section, and from observation, I have

$$\frac{v}{V} = \sqrt[3]{\frac{q}{Q} \cdot \frac{i}{I}} \dots (2), \text{ and } \frac{v}{V} = \sqrt[3]{\frac{av}{AV} \cdot \frac{i}{I}} \text{ or } \frac{v}{V} = \sqrt{\frac{a}{A} \cdot \frac{i}{I}}; \dots (3)$$

from which I obtain a new equation to the flow of water in uniform motion—that is, only when $V = v$. This is

$$\frac{I}{i} = \frac{1}{2} \left(\frac{a}{A} + \frac{q}{Q} \right), \dots \dots \dots (4)$$

which applies to water in canals in uniform motion, as in fig. 3, Plate III. The tendency of every river is to approximate in all parts of its course to a uniform mean velocity. The river carries sand and mud from the mountains to the sea along its channel at a nearly uniform rate. Increase of quantity of water flowing at any point balances decrease of slope throughout all rivers. A steamer ascending the Rhine meets a current descending at one velocity at different slopes. This is proved by the consumption of fuel being equal per mile from the sea to Mayence, except where back-water on one side increases velocity on the other, or where shallows retard the ship. I do not find (R the mean hydraulic depth) of value in calculations.

particles of water and move them is represented in a new manner. The sun's effect can be calculated similarly to the moon's.

The attraction of the moon when in a vertical line would not produce any horizontal or vertical movement in a particle of water below it; and the attraction could not produce a heap of water below it without the water being propelled from some point of the ocean on which the rays of attraction fell at an angle less than 90° ; and then I do not think the heap could exceed 2 inches in height*, for reasons which will be given hereafter.

The mass of the moon is equal to a sphere of 118.75 miles diameter of the same density as the earth, and situated at 3956 miles from the point to be attracted—that is, at the distance of the radius of the earth. The circle F near C (fig. 4, Pl. IV.) should be only one sixth of an inch if drawn to scale. It is shown in the position in which it would represent the action of the moon on the ocean if it revolved round C in a lunar month. Thus any point on the circumference of the earth must be attracted to the centre of the earth by an attraction greater than that of the moon to the same point in the ratio of 295520 to 1. For $60.263^2 = 3631$, and 60.263 is the mean distance of the moon from the earth; then the density of the earth is to that of the moon as 1.647 to 1, and the mass of the earth to that of the moon as 49.5 to 1. Then

$$3631 \times 49.5 \times 1.647 = 295520;$$

that is, the effect of the attraction of the moon in a particle on the surface of the earth (at the moon's mean distance) is only $\frac{1}{295520}$ of that arising from the attraction of the earth itself†.

The weight of any body on the earth would therefore be lightened in that ratio, or in the proportion of 1 grain to $4\frac{1}{2}$ gallons of water (70,000 grains to the gallon), the moon being

* This is a different case altogether from that of the estimate of collection of water at the equator; and the practical test given above is better than theory.

† This is calculated differently in a note to page 528, Herschel's 'Outlines of Astronomy,' 1873: the cube of the sun's distance is erroneously introduced into the calculation for finding the moon's maximum power to disturb the water in the surface of the earth; this brings the relative effect of the moon's attraction to $\frac{1}{125850000}$ of gravity, according to Herschel. This, however, is only $\frac{1}{12.5}$ of the real quantity. My calculation is according to the following law:—"Two such globes would (by the same proposition) attract one another with a force decreasing in the duplicate proportion of the distance between their centres" (Newton, page 24, edit. 1819). If Herschel's figures were correct, we should have tides of 3 inches on our coast instead of 12 feet in height. Also the fictitious moon F placed near C (fig. 4, Pl. IV.), to represent the effect of the real moon, would have only a diameter of 34.03 miles, and contain 20,629 cubic miles instead of the larger quantity mentioned by me in the text.

at its mean distance. The attraction of the moon I calculate to be one fifteenth greater at a point on the near side of the earth than at a point vertically below it on the off side at the antipodes. Every day the change of position of the moon with regard to the earth would affect all weights on the surface of the earth temporarily, but only to the extent of 1 grain in 61 gallons, a quantity which is not susceptible of measurement by a balance.

The effect of the moon's direct attraction is really very small on each cubic foot; but as it affects water at the bottom of the sea nearly as much as on the surface, it amounts to an enormous moving force when a stream 20,000 feet deep is set in motion. It is only perceptible to observation when motion is accumulated by composition at certain points, such as where there is a great composition of forces, as in soundings. Navigators do not observe the motion of the tide except near the coast. I calculate the amount in the following manner. If the effective force of the moon has to be multiplied eighty-four times to raise a 12-foot tide at a point of the coast where the sea is 238 feet deep, then the direct effect of the moon's attraction on water 238 feet deep would only be $\frac{1}{7}$ foot, or something under 2 inches. Thus I consider 2 inches is the greatest height that the moon could possibly raise the level of the sea under it with 238 feet depth of water, $\frac{8\frac{3}{4}}{84}$ of the elevation of 12 feet being the effect produced in deeper water by the moon and sun, transferred by the composition of forces to shallow water. The drawings (figs. 1, 2, and 3, Pl. IV.) from standard works on tides are therefore great exaggerations by their authors; and the descriptions accompanying them would lead any one to suppose a great heap of water could be rapidly accumulated in the central ocean by vertical attraction on deep water. The authors do not specify how the water is obtained, or whence it is comes, or the data by which they prove such a heaping up possible as is proposed by the equilibrium theory.

Time is the essence of such an operation, which, if done at all, must be completed in six hours, or a contrary current would set in. The heaping-up movement, to keep up with the rotation of the earth, would have, in the latitude of Brest, to make water flow at 11·3 miles per minute, which is clearly impossible. It is not, therefore, surprising that the effect of the tidal wave is hardly perceptible at oceanic islands, whereas, if figs. 1, 2, and 3, Pl. IV., were correct, it ought to be as large there as on the mainland coast.

Fig. 1, Pl. IV. is an explanation of the tides copied from the 'Penny Cyclopædia;' figs. 2 and 3, Pl. IV., are from Dr. Lardner's 'Astronomy,' pp. 324-5; and my own view is given in fig. 4; so that the reader may compare the different theories.

If fig. 4, Pl. IV., is correct, there can be no great heaping up of water or any tidal wave generated in one direction, as has been sometimes assumed; for I show that the action of the tide is a reciprocating action, and has as much motion from west to east as from east to west.

The assumption of a great heap of water travelling in one direction, or producing a certain amount of retardation of the rotary movement of the earth, quite unbalanced by acceleration, has been taken as a serious fact; many writers of reputation have supposed that the rotation of the earth must be affected by this hypothetical wave-action in one direction.

My view of the general theory of the tides (fig. 4, Pl. IV.) differs materially from those generally accepted; and I cannot understand the existence of an intumescence (shown in figs. 1, 2, and 3, Pl. IV.) under the moon at all if the subject is treated in the ordinary manner of reasoning.

I entirely disbelieve in tidal action having the smallest effect on the rotation of the earth. It is a balanced action. The sun might produce currents by unequally heating water, which might affect the surface-level of the sea and cause inequalities; but of this there is no positive evidence. I show by fig. 3, Pl. III. that, under certain circumstances observed, a current may travel against the slope of the surface. This I noticed in your *Journal* in 1853. The existence of a current is of itself, therefore, no proof of what is the direction of the slope of the surface. I find that an elevation of level of 2 inches on the east maintained over the west side of the Atlantic, or *vice versé*, where water is very deep, would generate a current of 3 feet per minute in the ocean in the direction of the slope, supposing the Gulf-stream did not intervene and there was no tidal action. The western water would take 5 years to cross the Atlantic at a speed of 3 feet per minute, to reestablish equilibrium. If the difference of level were produced by luni-solar action, it would cause no current until the force creating it was withdrawn.

If 58 miles per hour is the greatest velocity a surface-wave could travel at in the deepest part of the Atlantic, such an intumescence, even if maintained, would have only proceeded 348 miles before the moon's influence would be exerted against its motion. The great earthquake-impulse of Lisbon in 1755, proceeding through deep water, did not travel to Barbadoes faster than 6 miles per minute; and an intumescence of equal force or impulse created on the W. coast of America, in the latitude of Brest, would meet contrary luni-solar attractions when half-way across the Atlantic. No intumescence could be raised in deep water without forming a gap below.

In fig. 4, Pl. IV., I take a particle of water at certain points on opposite hemispheres, and show the direction of the resultants of the two forces (the moon and earth's attraction) and the probable deflection of the moon's attractive rays in passing through the earth. Sir J. Herschel (p. 528) only gives one position on his Plate (p. 464), and leaves it to the reader to try the position of particles on other parts of the circle and find their direction. I have tried to do so according to his rule, and find that his diagram would only apply to the moon's attraction by the earth at such an immense distance that gravity could be considered as acting to and from single points, viz. the centres of the two attracting bodies, and surface attractions need not be considered. Now the case of the tides is clearly that of a point or particle at the surface of the earth being attracted by the two *centres* of the earth and moon. Herschel's figure (p. 464) is not appropriate to the conditions of the tides; for it has no special relation to surface attraction at all. If it proved any thing about tides, it would prove there would only be a tide every twenty-four hours. On the contrary, in fig. 4, Pl. IV., I take into account the deflection of the rays of attraction on entering the earth, and find that, if they pass through the earth with the rapidity of light, when they reach the other hemisphere they cause a twelve-hours tide, simultaneously produced to that on the opposite side of the globe.

I have proved by analyzing the experiments on waves by J. S. Russell and by Darcy, that the velocity measured in feet per second of any wave when generated does not exceed three times the cube root of the depth of the water it was generated in, measured in feet; that is, $v = 3\sqrt[3]{p}$, a new formula, which answers both for small and great depths, the usual formulæ giving results much too high for waves generated in deep water.

Mr. W. Parkes (Phil. Trans. 1868) suggests that the alternate tides are produced in different hemispheres, and that the evening tide which reaches Kurrachee twelve hours after the morning has travelled a greater distance. This does not seem probable; nor does he give any evidence on the point. Then, with regard to the diurnal variations of the two tides, Mr. W. Parkes (p. 686, says the diurnal inequalities disappear when the attracting bodies are in the plane of the equator. It appears to me, from the observations at Kurrachee, that when the diurnal irregularity of the high-water points is at its maximum, the diurnal irregularity of the low-water points is at its minimum, and *vice versâ*. Curiously enough, at Kurrachee the mean diurnal low-water irregularity is about 2 feet, against about 1 foot (the mean diurnal irregularity of the high

water). That is, instead of $\frac{1}{15}$ of the height of this lunar portion of the tide representing the diurnal irregularity (which I consider is the mean of the world), at Kurrachee the tides are so exceptional that the diurnal irregularity amounts at low water to $\frac{1}{3}$, and at high water to $\frac{1}{8}$ of the total height on the average of the tides for a month. This opposite action cannot be owing to the position of the attracting bodies in the respective tides being in the plane of the equator.

I would observe that the deep central ocean without any vertical tidal movement or tide-wave observed is certainly forty times as large as the shallow coast-sea, where a rise of tide is observable. The composers of figs. 1, 2, and 3, Pl. IV., seem to regard the coast alone, which I consider the exception. They do not seem to think of the tidal conditions in the great mass of the ocean at all in forming their theory.

It appears from figs. 1 and 2, Pl. II., and fig. 1, Pl. III., from observation, that the level of the sea at high water, even in the tidal estuary of the Thames, is only raised 5 feet, and in that of the Clyde 1 foot, above the central ocean.

The high-water points from Falmouth to Sheerness are nearly level; they only deviate 1 foot in 500 miles from a straight line.

The fact has not been sufficiently considered, that water in open channels can be moved under certain conditions against gravity, and that the great central mass of the ocean swinging backwards and forwards every six hours is one of the forces that can easily overcome gravity when producing a slow current. As early as 1853 I gave a drawing in your Journal (p. 259) of the bottom-water outside the bar of the Mississippi being raised to the surface 16 feet against gravity by the current of fresh water flowing outwards, partly impelled by gravity (propulsion) and partly sucked or drawn by the tidal water in front of it (aspiration): see fig. 4, Pl. III. I still believe that the tidal current acts like the piston of a pump, and reduces the pressure in its rear, and draws or sucks out the coast-water after it in the ebb-tide, and pushes the water back again to fill up the gap when its motion is reversed by the luni-solar force in the flowing tide. I first observed evidence of this action on the bars of rivers, and represent it in fig. 2, Pl. III. As the water in the Mississippi is 100 feet deep at a comparatively short distance behind the bar BC, and is in motion from* top to bottom, the lower water is evidently drawn over the bar and up an ascent of 84 feet against gravity, by the pressure of the water at BC on the bar being reduced by the tide or mass of oceanic water moving steadily before it. Motion, of course, ensues in the direc-

* Humphrys and Abbott record rapid motion at the bottom at Carrollton, page 149.

tion of least pressure; and that happens to be, as far as the upper part of the water is concerned, against gravity, and is similar to what happens in a mill-race (fig. 3, Pl. III.). This action, which takes place at the mouths of all large rivers, is a clue to the tidal movements. It is true that in the Severn at Beachley the high-water level of spring tides is 25 feet above the Ordnance Datum, and 21 feet above the level of the central ocean. But this is an exceptional case that can be explained. At Beachley at high water the cross section is 400,000 sup. feet, and at low water only 25,000 sup. feet (see fig. 1, Pl. II.).

At Stonebench, 36 miles higher, there is only a cross section of 240 feet, and a depth of 3 feet at low water.

The cross section at Beachley is not a tenth of the section a few miles lower down in the Bristol Channel.

The exceptional height of the tide there is solely due to the funnel shape of the channel, caused by the hard rocks that prevent the tideway being excavated to the usual form. This exception proves the rule. I compare the ebb-tide to the action of a mill-stream, thus:—

Fig 3, Pl. III., represents, from actual observation, a case of water moving against gravity in an open channel, and against the direction of the slope of the surface of the water. The stream of water passing over the weir at B falls in a thin stream at great velocity to C₁. Here it changes its direction and the current is against the slope of the surface, viz. towards D₁ instead of towards C₁, which would be the direction of gravity. The stream D E in uniform motion reduces the pressure at D and draws the water from C after it—just as the great central oceanic stream represented in figs. 1 and 2, Pl. II., and in fig. 1, Plate III., draws the coast-water westwards and forms a gap which is filled by the tide, the oceanic stream having reversed its direction in six hours, as shown in fig. 4, Pl. IV., by luni-solar attraction pushing on to the coast-line the flowing tide.

My explanation of the luni-solar attraction in fig. 4, Pl. IV., is placed adjoining the drawing.

It will be observed I omit in the diagram (fig. 4, Pl. IV.) the customary theoretical intumescences opposite to each other moving with the moon, but through each of which every part of the earth is supposed to pass daily, as in figs. 2 and 3, Pl. IV., and I show an alternating tide in the ocean itself instead, in fig. 4. I confess I cannot follow the supposed changes of form of the ocean-surface in figs. 1, 2, and 3, Pl. IV., nor imagine either that such movements could possibly occur, or that they would at all describe the tidal changes at any point of the globe as known to observers. I remark, the writers give no dimensions of the intumescences, or calculate the force to convert the circle *ab* into

the ellipsoid $a_1 b_1$. M. Arago very justly wrote that "details are the touchstones of theories;" and all details are absent in the well-known articles on this subject. In fig. 1 the earth is represented as a circle, and the ocean as an ellipsoid. In figs. 2 and 3 the water is the circle, and the earth the ellipsoid.

According to figs. 1, 2, and 3, Pl. IV., the velocity of the tide would be equal at high, low, and half tide to any observer on the earth.

The greatest action, on the contrary, is shown to be at the half tide, both ebb and flowing, in my diagram, fig. 4, Pl. IV. Captain Beechey* remarked that the velocity of the current was greatest at half tide; and this disproves any theory in which the tide is supposed theoretically equally strong at all parts.

Dr. Lardner† explained his diagrams, figs. 2 and 3, Pl. IV., by stating that the moon forces down the water at the sides at right angles to her direction, and raises it at the two ends of its diameter pointing to her. In figs. 2 and 3, Pl. IV., the moon pulls the water in one hemisphere and pushes it away in the other. This is the first time that the property of repulsion or forcing has been attributed in this manner to the heavenly bodies. He shows an exactly opposite direction of forces on the near and far side of the earth produced at the same moment by the moon.

Notwithstanding any language that may be used to make figs. 1, 2, and 3, Pl. IV., appear to satisfy the actual tidal conditions, it will, I think, be evident to the reader that the positions of the forces as drawn are not in accordance with the ordinary laws of mechanics. Herschel refers the reader to his drawing (*Astronomy*, p. 464), in which the retardation and acceleration of the moon in its elliptic orbit round the earth at a mean distance sixty times the radius of the earth is proved to be according to the law of equal spaces being described by the moon in equal times, and consequent variation of motion.

The reader is recommended by Herschel to prove the acceleration or retardation of the tides from the diagram (p. 464), which is really impossible, as the figure relates to an entirely different case, and is in a part of his book relating to the motions of the moon. It is admitted that the moon is a free body attracted at a great distance by the earth, and made to move at varying velocities round the earth in a lunar month at a rate dependent, among other causes, upon the relative weights and distances of the moon and earth, and the original impetus and angle at which these bodies were projected into space. The tidal water, on the contrary, is held as an inseparable mass of fluid reposing in a basin of earth; and it travels at the same uniform speed of rotation as

* *Phil. Trans.* 1851, p. 711.

† Lardner's *Astronomy*, p. 336.

the earth, except when modified in velocity to a very small extent by the attraction of the celestial bodies, producing the tides. The two cases are not parallel, and the diagram mentioned by Herschel is certainly not applicable to the tides at all. The oceanic water has no tangential force independent of the earth; for two points in the ocean, reaching 180° , opposite to each other are at the same distance from the centre of the earth, and are in exact equilibrium if the difference of tidal action is left out of consideration. I think the authors have not taken into consideration the fact that the rays of attraction from the moon to the water, when the water is screened from the moon by the earth, would pass through the earth, in order to reach the opposite side, *not* in a straight line. They would not only lose force of course as the square of the distance increased, but I think no ray or vibration or impulse or line of attractive force could fall upon or pass through a curved body such as the earth at an acute angle without being in some way deflected or diverted from its direct course in passing through the earth to water on the other side; and the reverse is true. I believe a vibration of any kind, or attraction-ray, would have to be modified in its direction or bent at the point of contact, so as to enter the surface of the earth at a right angle to a tangent of the curve at the point, as shown in fig. 4.

My own view (Pl. IV. fig. 4) shows slack water at the turn of the tide both near high and near low water; but of course these events do not always coincide. When the luni-solar attraction-rays fall near A and B (fig. 4, Pl. IV.), they are evidently deranged and deflected so as to produce very little effect; in fact the state of the tide near those points is what might be called bordering on motion, which state accords with observation. Although the angle $M_1P_1R_1$ is only $1''\cdot07$, or two thirds of the angle $M_{11}P_{11}R_{11}$, yet the actual effect in producing tidal motion is much less than that proportion. For at P the tidal motion is being reversed, and the speed of the flowing tide has to be created from slack water; while at P_{11} the effect of attraction from M_{11} is to accelerate the tide then moving freely and in the same direction as it has been between P_1 and P_{11} , and also in the line of the earth's rotation. The actual effect in velocity theoretically is quite four times as much in the half hour near P_{11} as at that near P_1 ; and this accords with observation. The angle of the resultant R_1 is found thus. The angle M_1P_1C is $88^\circ 30' 4'' = 318604$ seconds.

The force M_1P_1 is to CP_1 as 1 to 295,520; \therefore the angle $M_1P_1R_1$, the resultant, equals $\frac{318,604}{295,520} = 1''\cdot07$. In the same manner the angle $M_{11}P_{11}R_{11} = 1''\cdot66$. The depth of the Atlantic (five miles) favours movement very much, as there is room for the force in the direction $A R_{11}$ to be transferred easily into the direc-

tion $P_{\parallel}E$, which is that of the line of rotation. In an inland sea like the Mediterranean, where the deep water is 6000 feet, the movement of the tide on the coast is often not more than 1 foot. There are 7-foot tides in one or two places in the Mediterranean where the contour of the coast is like our Bristol Channel.

If the deep water in the Mediterranean Sea were one twelfth of the depth of that in the Atlantic, we should expect a 1-foot tide there in place of a 12-foot, according to the law of composition of forces*. The peculiar circumstances of the Mediterranean make the tides much smaller than I should have calculated from the experience of the Atlantic. Taking 6000 feet as the basis of the deep water and 9 inches as the tide, diffusion of the tidal force from deep central to surrounding shallow coast-basins seems to absorb $\frac{2}{3}$ of the force. The proportion of deep water is very small.

The Mediterranean standard of rates of depths of ocean to height of tide along its coasts, seems to match more with Pacific and west-coast-of-America standards than with the observations on the eastern Atlantic coast. The European tides seem exaggerated, even when compared with the east coast of America. This may be explained by less diffusion of tidal force and the contour of the sea-bottom on our coast being more favourable to receiving impulses giving velocity to the coast-water than that on the east coast of America. For want of space I have hardly been able to allude to the solar influence of the tides, which differs in some respects from the lunar relations.

The diurnal and semidiurnal tides are known to vary about 4 inches in an 8-foot tide. Then, supposing that 2 feet of the 8 feet is caused by solar attraction, the variation is one fifteenth of the height due to lunar action. If the tide generated in deep water is twelve lunar hours reaching a part of the coast, the greater alternate twelve-hours' tide will become the lesser of the two. There are many other considerations to take into account which materially modify the size of the alternate tides at different parts of the month; and I do not put forward my own view with any pretensions to improve the prediction of tides, which indeed is already perfectly done by the machine invented by Sir W. Thomson and Mr. E. Roberts of the Nautical Almanac Office. What I wish to do is to give an explanation of a theory of the tides which shall accord with the physical facts. Supposing the point E is thirty diameters of the earth from the moon on any day, then the point W will be 31. Then, the attraction

* In the Admiralty Tide Tables there are only tides at ten places in the Mediterranean recorded. The highest spring tide is 7 feet, and the average 4.3 feet. Admiral Spratt, F.R.S., has just informed me that the average of all spring tides in the Mediterranean is from 9 to 10 inches, perceptible within three days of the highest tide. It is evident to me that the tide in that sea is generated in basins, so that it is diffused in getting to the coast, by which $\frac{2}{3}$ of the proper height must be lost.

being inversely as the square of the distance, the force at E will be to that at W as 31^2 to 30^2 , or as 961 to 900 (that is, one fifteenth greater). This calculation ought to agree with observation at ports where the variation in height each alternate tide is eliminated from other disturbances, and where there are no exceptional circumstances, if this is a correct explanation of the difference between the height of the diurnal and semidiurnal tides (which I term the near-side and far-side tides).

The luni-solar attraction-rays in passing through the earth may encounter changes from fluid to solid substances having surfaces not at right angles to the incident rays; and the rays would not then follow straight lines, although I have for convenience represented them as straight in fig. 4, Pl. IV.

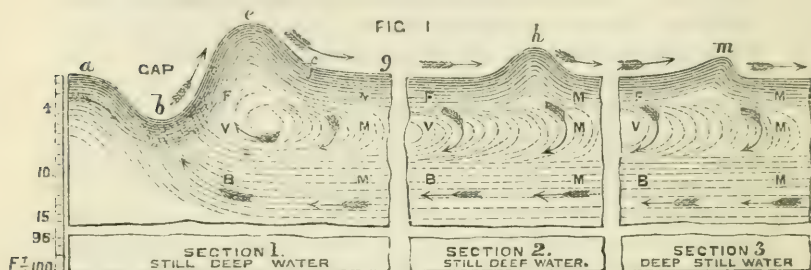
Those rays passing through the higher parallels of latitude far from the centre might affect the tides apparently in an irregular manner. These changes of direction might explain why, in order to predict with great accuracy the height and time of the tide at some stations, Sir W. Thomson and Mr. E. Roberts have been obliged to employ twenty-seven fictitious stars instead of only the number to express the effects of the moon and sun's various positions.

The different currents that occur, causing different establishments at ports near each other, seem to indicate movements of masses of water apparently at different angles to each other. These motions can be illustrated by an experiment in the injector. In the water-pipe, at right angles to the body of the injector (where steam is at 101 lbs. pressure), there is a partial vacuum, say equal to 2 inches of water. I find that the motion of the steam will increase its own pressure 1 lb. by friction against the metal instrument. The steam travelling with great velocity deflects the water-current and bends it into its own direction, and forces water into the steam-boiler, where the pressure is 100 lbs. The water-pipe is all the time open to the atmosphere and to the boiler two ways through the injector; but little steam escapes through the open water-pipe. The barometer is another instance. The column of mercury ought to lengthen if that instrument registered the absolute weight of the atmosphere alone, when the column of air is loaded with vapour. The motion of the vapour in the act of condensing, however, generates currents and produces motion of particles in a direction across the column. This reduces the pressure of the column on the cistern of the barometer; and therefore the column shortens for motion instead of lengthening for weight. Motion in main water-pipes reduces pressure in branches where there is no motion.

Currents in motion in different directions, owing to different temperatures or other causes, affect the tidal currents materially,

and prevent the tide-gauge ever registering the tidal influences alone at any point. This is the cause of different establishments at neighbouring ports apparently in similar position as regards the luni-solar influences.

Fig. 1 shows that the area of the gap formed when the wave



F.M. LINES OF HORIZONTAL FORWARD MOTION OF PARTICLES WITH VERTICAL MOTION.

V.M. LINES OF VERTICAL MOTION OF PARTICLES WITHOUT HORIZONTAL MOTION.

B.M. LINES OF HORIZONTAL BACKWARD MOTION OF PARTICLES WITH VERTICAL MOTION.

was generated is the limit of horizontal movement of particles throughout the run of that wave. Experiments show that if waves artificially produced for experiments continue the same height their velocity diminishes, and if their height diminishes they may keep up their velocity. It is impossible to keep up both the velocity and height of any wave a long distance. If it were possible it would involve perpetual motion, as the wave is resisted by the air above and by the water in which it vibrates.

Let v represent velocity of the motion of a wave measured in feet and the time (a second) in which its crest passes a fixed point, and p the depth of the water in feet; then by means of the formula $v = 3\sqrt[3]{p}$ the actual velocity found by experiment may be predicted as accurately as by the usual formula $v = \sqrt{gh}$ *. The latter formula appears extremely incorrect for great depth, as it indicates impossible velocities for waves. If the depth of the Atlantic was 21,952 feet, the greatest velocity that could by any means be given to a wave would be 84 feet per second, or 58 miles per hour; for if $v = \sqrt[3]{3p}$, then from this we have $84 = \sqrt[3]{3 \times 21952}$, that is, 84 feet per second is the maximum ve-

* The gravity formula, $v^2 = 2gh$, only applies where there is no resistance to motion. It is of no use in cases of uniform motion. My new formula (page 205) gives the due effect of weight on velocity. Thus in a river or a glacier with sixty-four times the quantity (or weight) flowing or sliding, the velocity would increase four times at the same slope. This law explains why in the glacial period frozen rivers reached such low levels, and why denudation was so large in the pluvial period, as destructive effect is in a high ratio to the velocity.

locity of the wave, instead of the impossible velocity of 800 miles per hour suggested by some authors.

An earthquake might transmit a blow through the deep water in the ocean at six miles per minute, as a wave was formed at Barbadoes 3000 miles from the supposed origin of the shock in 585 minutes after it was observed at Lisbon. Michell, in 1755 wrote "when the bar at the mouth of the Tagus was seen dry from shore to shore, then suddenly the sea, like a mountain, came rolling in." When this blow struck a distant coast below the level of the sea, it would be reflected and cause the sea to ebb from the coast first. Then when the force which heaped up the water away from the shore diminished by work done upon the water in raising up the level of the sea, a great wave would be moved shorewards by gravity. The first announcement of the approach of an earthquake-wave is the ebb of the water*, not a surface-wave.

If a great surface-wave were generated by an earthquake, it would not travel very far, but would soon diminish in height and speed, and would not be preceded by a wave in an opposite direction.

In some careful experiments in a course of one fifth of a mile † a surface-wave lost five sixths of its height. A powerful shock or impulse could possibly be communicated through deep water, like a blow through a solid body, an immense distance with great velocity; but that is not the case of a surface-wave at all.

There is, therefore, a great distinction between primitive tidal impulses and the secondary waves that accompany or follow them, or the movements in coast-water produced at distant places and times by means of the composition of forces. The tidal impulse is communicated rather in the manner motion is conveyed from a steam-engine through mechanical gearing, such as drivers and followers, and where there is lost time and lost motion between the teeth of the driving-wheels, or bands and pulleys, or levers, or other parts of the apparatus through which the movement is communicated from a prime mover to some distant point.

Thus the piston may have commenced its down-stroke before the effect of the former up-stroke had reached the extremity of the shafting. This lost motion is very perceptible in figs. 1 and 2, Plate II., and fig. 1, Plate III.

The particles of water may revolve along their axes; or all the vibrations may not be effective, some of them neutralizing others, and for a short time destroying the impulse of the central tide-generating force, soon to be renewed.

The hours at which high water arrives are written against the

* Michell, Phil. Trans. 1755.

† Brit. Assoc. 1838, p. 465.

names of the towns situated on the coast or river-bank in Plates II. and III.

In fig. 1, Plate II., a point is assumed in the Atlantic 300 miles from the Land's End, where the high- and low-water level is assumed to be invariable, and where the mass of the ocean water is supposed to move east and west very slowly in alternate and opposite directions in each tide.

When the flowing tide is moving a ship 3 miles an hour, there is 360 miles difference in distance, and 6 hours' time, between high water at Plymouth and Dover; therefore the lost motion is 18 miles out of 360, or 5 per cent. The impulse received at Plymouth from the central slowly moving oceanic water must have been transmitted through the deeper water at a much higher rate, but only reaches Dover after travelling at 60 miles an hour. Then the impulse is transmitted from Dover to London at the rate of 120 miles in three hours, or 40 miles an hour, the tide only taking a ship 9 miles in 3 hours; so that the lost motion is 9 miles out of 120, or 7 per cent., the difference of time between Plymouth and Dover (21 minutes) not being taken into account.

I have allowed a slope of 1 foot in Plates II. and III. to bring the water to the mouth of the Clyde, and 5 feet to bring the water to Falmouth from the Atlantic.

If the level of the ocean were kept up above its due level only 2 inches between the western and eastern boundary of the Atlantic deep water, that slope would suffice to create a current of 3 feet per minute in the whole mass of deep water. This is supposing the law of velocity followed the ratio I observe in smaller cases. If the two inches were only water heaped up in consequence of, or by the luni-solar attraction, it would create no current at all while that attraction continued.

As the effect of the moon on the oceanic water is only equal to that of a sphere of 118·75 miles in diameter, equal in mean density to the earth, placed near and revolving about C in a lunar day, it occurred to me that some geological difficulties, such as the evidence in the Crag and Quaternary deposits of the tides in the Quaternary period being three or four times as large as at present, might be explained by periodic changes of position of part of the interior of the earth, rather than by supposing great changes in the distance of the moon from the earth. Also the quantity of water in the ocean can only be the difference between that of the vapour held in the atmosphere or condensed into snow or ice on the land, and the quantity of water or vapour of water mechanically or chemically combined with the strata of the earth. These are quantities capable of enormous variation in geological periods under different conditions. There is also a

periodicity about the alternation of land and water surfaces, particularly in the Carboniferous period, which might be explained by slow changes in long intervals of the disposition of the solid and fluid internal substance of the earth with regard to and about its centre.

A slow circulation of an eccentric mass of fluid may occur in the interior of the earth, and gases may periodically pass from one part of the solid portion to another, their place being supplied by fluids, attracting the ocean unequally.

Unequal attraction from variable subterraneous inequalities would affect different points of the surface and raise the water-level on one part for long periods and depress it on alternate and opposite points to an equal extent. The theory of inconstancy of rainfall and of fluctuation of the sea-level in geological periods is gaining ground since I first advanced these views (in 1853) in this Journal, in a paper entitled "Fluctuations of the Sea-level in stated Periods of Time."

We must not gauge our interpretation of nature by the present temperature, rainfall, or tide-gauges, but from the actual evidence presented in the strata themselves.

In conclusion, if all the lines of lunar attraction M_p , M_{pp} , &c. (fig. 4, Pl. IV.) were continued through the earth without deflection from a straight line, then there could be only one lunar tide in the twenty-four hours; for all the water on one side of the axial line, E C W or half the globe, moving in the direction of the rotation of the earth, would be accelerated, and all the water in the other half, E B W, of the globe would be retarded, as the attraction of the moon in that half would be contrary to the direction of the rotation of the earth. The fact of the tides occurring every twelve hours is a proof that the view I have put forward of the deflection of the attracting rays in their passage through the earth is a correct one. The twelve-hour tides on the opposite side of the earth to the moon are physical proofs that attraction-rays are deflected. If not, there could be no such effect of attraction on the ocean as is shown by the twelve-hour tide. The theoretical differences of one fifteenth of the height of alternate tides I believe accord with observation, taking the average of the world.

According to Professor Stokes, any solution of a problem that satisfies all the conditions must be the true one. I believe the solution suggested in this letter conforms entirely to the facts, and that the deflection-theory, now, I believe, first proposed, is true.

Yours truly,

A. TYLOR.

P.S. With regard to the new equations to the flow of water in page 205, I use coefficients for different materials of channels: see note to Pl. III. fig. 3.

XXXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 153.]

February 5, 1874.—Joseph Dalton Hooker, C.B., President, in the Chair.

THE following communication was read:—

“On a Self-recording Method of Measuring the Intensity of the Chemical Action of Total Daylight.” By H. E. Roscoe, F.R.S.

The object of the present communication is to describe an instrument by which the varying intensity of the chemically active rays, as affecting chloride of silver paper of constant sensitiveness, can be made self-recording. The method described by the author in the Bakerian Lecture for 1865, although it has been the means of bringing into notice many important facts concerning the distribution of the sun's chemical activity throughout the atmosphere, as well as in different situations on the earth's surface, has not as yet been introduced as a portion of the regular work of meteorological observatories, owing to the fact that, in order to obtain a satisfactory curve of daily chemical intensity, at least hourly observations need to be made, and this involves the expenditure of more time and labour than it has been found possible to give. In the present communication a method is described, which, whilst preserving untouched the principles and accuracy of the former method, reduces the personal attention needed for carrying out the measurements to a minimum, and thus renders its adoption in observatories possible.

According to this plan, a constant sensitive paper is exposed by a self-acting arrangement for accurately known times, at given intervals throughout the day. The insolation apparatus stocked with sensitive paper is placed in position either early in the morning of the day during which the measurements have to be made, or on the previous night, and by means of an electric communication with a properly arranged clock, the sensitive paper is exposed every hour during the day, so that, in the evening, the observer has only to read off, in the ordinary manner, the hourly intensities which have been recorded on the paper during the day.

This self-recording arrangement, though apparently simple, involves points which have rendered its successful completion a somewhat difficult matter, owing, in the first place, to the great variations which occur in the chemical intensity of total daylight in different places, at different times of the day, and in different periods of the year; and secondly, owing to the fact that, in order to be able to estimate the chemical intensity, the coloration acquired by the paper must reach, but not much exceed, a given tint. It becomes necessary therefore that on each occasion when an observation is needed, the sensitive paper should be exposed me-

chanically, not once, but for several known but varying intervals of time quickly succeeding each other ; so that whatever may be the intensity of the total daylight (supposed during these intervals to remain constant), some one at least of the several exposed papers will possess the requisite shade. This is accomplished by a duplicate arrangement of a clock and insolation-apparatus, by means of which disks of the constant sensitive paper are exposed each hour for successive known intervals of time, varying from two to thirty seconds. After an interval of an hour, another set of disks are exposed for the same series of intervals ; and these series of insolutions are repeated once every hour during the day. The mechanical arrangements for effecting this with accuracy are fully described in the paper. On unrolling, at the end of the day, the strip of sensitive paper which has served for the exposures, black disks showing where the paper has been stationary for the hour are seen ; and between each of these are found ten circles variously tinted, from that, probably, scarcely visible, which was exposed for two seconds, to that, perhaps too dark to read off, which was insolated for thirty seconds. Amongst these, some one at least, will be found of such a shade as to enable it to be read off by the monochromatic soda-flame, on a graduated fixed strip, as described in former communications.

A new method of calibrating the fixed strips of standard tints necessary for these measurements is next described ; and the question as to the possibility of preparing constant sensitive paper in long strips instead of in large sheets is next experimentally discussed, the result of the examination being that it is possible to prepare silvered paper in long narrow strips such as are used in Morse's telegraph-apparatus, so that it shall throughout its length preserve the standard sensitiveness.

The time during which the disks of constant sensitive paper are exposed is next ascertained for each instrument by a chronograph.

During wet weather the insolator is covered by a semicircular glass shade ; and the value of the coefficients for refraction and absorption due to this glass shade is determined.

The latter portion of the communication contains the results of a series of comparisons of the curves of daily chemical intensity obtained (1) with the hand-insolator, and (2) with the self-recording instrument. Comparisons of this nature were made during the months of May, June, and July, 1873, by simultaneous hourly determinations in the neighbourhood of Manchester according to both methods. Of these observations, six full days are selected ; and the tables and curves accompanying the communication show the close correspondence of both sets of observations. The integrals of total chemical intensity for these days are also given, and exhibit as close an agreement as, from the nature of the experiments, can be expected.

Feb. 12.—Joseph Dalton Hooker, C.B., President, in the Chair.

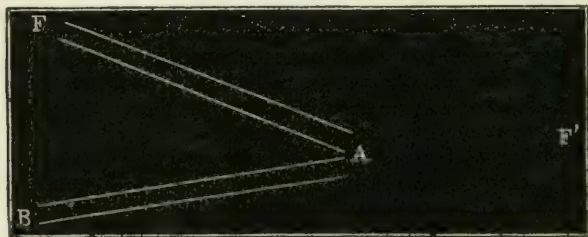
The following communication was read :—

“On the Division of a Sound-Wave by a Layer of Flame or heated Gas into a reflected and a transmitted Wave.” By John Cottrell, Assistant in the Physical Laboratory of the Royal Institution.

The incompetency of a sound-pulse to pass through non-homogeneous air having been experimentally demonstrated by Dr. Tyndall, and proved to be due to its successive partial reflections at the limiting surfaces of layers of air or vapour of different density, further experiments were conducted in order to render visible the action of the reflected sound-wave.

The most successful of the various methods contrived for this purpose consists of the following arrangement. A vibrating bell contained in a padded box was directed so as to send a sound-wave through a tin tube, B A (38 inches long, $1\frac{3}{4}$ inch diameter), in the direction B F', its action being rendered manifest by its causing a sensitive flame placed at F' to become violently agitated.

The invisible heated layer immediately above the luminous portion of an ignited coal-gas flame issuing from an ordinary bat-swing burner was allowed to stream upwards across the end of the tin tube B A at A. A portion of the sound-wave issuing from the tube was reflected at the limiting surfaces of the heated layer; and a part being transmitted through it, was now only competent to slightly agitate the sensitive flame at F'.



The heated layer was then placed at such an angle that the reflected portion of the sound-wave was sent through a second tin tube, A F (of the same dimensions as B A), its action being rendered visible by its causing a second sensitive flame placed at the end of the tube at F to become violently affected. This action continued so long as the heated layer intervened; but upon its withdrawal the sensitive flame placed at F', receiving the whole of the direct pulse, became again violently agitated, and at the same moment the sensitive flame at F, ceasing to be affected, resumed its former tranquillity.

Exactly the same action takes place when the luminous portion of a gas-flame is made the reflecting layer; but in the experiments above described, the invisible layer above the flame only was used. By proper adjustment of the pressure of the gas, the flame at F' can be rendered so moderately sensitive to the direct sound-wave,

that the portion transmitted through the reflecting layer shall be incompetent to affect the flame. Then by the introduction and withdrawal of the bat's-wing flame the two sensitive flames can be rendered alternately quiescent and strongly agitated.

An illustration is here afforded of the perfect analogy between light and sound; for if a beam of light be projected from B to F', and a plate of glass be introduced at A, in the exact position of the reflecting layer of gas, the beam will be divided, and one portion will be reflected in the direction A F, and the other portion transmitted through the glass in the direction F', exactly as the sound-wave is divided into a reflected and a transmitted portion by the layer of heated gas or flame.

Feb. 19.—Joseph Dalton Hooker, C.B., President, in the Chair.

The following communication was read:—

“On an Instrument for the Composition of two Harmonic Curves.” By A. E. Donkin, M.A., F.R.A.S., Fellow of Exeter College, Oxford.

The interest in such compound curves lies in the fact that, as a simple harmonic curve may be considered to be the curve of pressure on the tympanic membrane when the ear is in the neighbourhood of a vibrating body producing a simple tone, so a curve compounded of two such simple harmonic curves will be the curve of pressure for the consonance of the two tones which they severally represent, and thus the effect on the ear of different consonances can be distinctly represented to the eye.

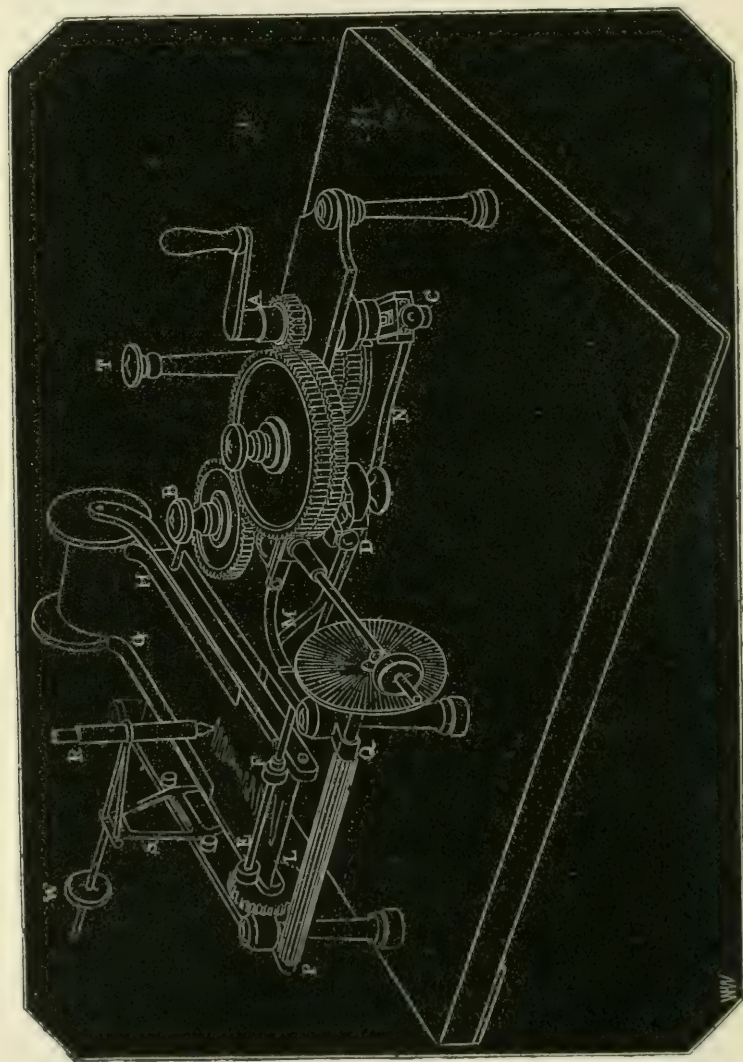
If the motion of a point be compounded of rectilinear harmonic vibrations and of uniform motion in a straight line at right angles to the direction of those vibrations, the point will describe a simple harmonic curve.

Thus a pencil-point performing such vibrations upon a sheet of paper moving uniformly at right angles to their direction would draw such a curve.

The same kind of curve would also be drawn by keeping the pencil fixed and by giving to the paper, in addition to its continuous transverse motion, a vibratory motion similar and parallel to that which the pencil had; and if the motion of the latter be now restored, a complicated curve will be produced whose form will depend on the ratio of the numbers of vibrations in a given time of the pencil and paper, and which will be the curve of pressure for the interval corresponding to this ratio.

The manner in which these three motions are combined in the machine is as follows:—Two vertical spindles, A and B, revolving in a horizontal plate carry at their lower ends each a crank, C and D, and at their upper ends each a wheel cut with a certain number of teeth; these two wheels can be connected by means of an intermediate one, as is seen in the figure; and since either wheel of the pair can be replaced by another with a different number of teeth, the relative angular velocities of the spindles can be regulated at pleasure. The paper upon which the curve is to be drawn is carried upon

a rectangular frame, E F G H, capable of sliding horizontally up and down in a direction parallel to that of the plane passing through the



spindles. This frame has a pair of rollers, E F and G H, at each end connected by tape bands, between which the paper passes as the rollers turn. In order to give a motion of revolution to the rollers, a wheel, L, is fixed upon the axis of one of them whose teeth gear

into those of a pinion, P Q, alongside which the frame slides, and which is itself driven by one of the vertical spindles. A connecting-rod, D M, is carried to the frame from the crank of this spindle, so that upon turning the latter a vibratory motion is given to the former; and since the transverse motion of the paper also depends upon the same spindle, a fixed pencil-point resting on it would draw a simple harmonic curve whose amplitude would depend on the radius of the crank, and wave-length on the transverse speed of the paper, which can be regulated at pleasure by means contrived for the purpose*.

A vibratory motion similar and parallel to that of the frame is given to a small tubular glass pen, R, so arranged as to move with its point lightly resting upon the paper. This motion is communicated by a connecting-rod, C N, from the other crank, which is carried underneath the sliding frame and jointed to the lower end of a small vertical lever, S, to whose upper end the arm carrying the pen is attached.

The weight W serves to regulate the pressure of the pen on the paper, as it can be screwed in or out. T is merely a pillar upon which the change-wheels can be placed for convenience.

If the pair of wheels on the spindles are now connected by the intermediate one, it is plain that, upon turning either of the spindles by a winch provided for the purpose, the two motions of the paper will be combined with that of the pen, and the curve drawn will be that composed of the two simple harmonic ones which would be the result of separately combining the harmonic vibrations due to each crank with the transverse motion of the paper. Thus, if m and n are the numbers of teeth on the pair of wheels respectively, the equation to the resultant curve will be

$$y = \sin mx + \sin nx.$$

This equation implies not only that the radii of the cranks are the same, but also that they start parallel to each other and at right angles to the vertical plane passing through their axes: both these conditions can, however, be altered; and therefore the general form of equation to the curves which the machine can draw will be

$$y = a \sin (mx + \alpha) + b \sin (nx + \beta),$$

where a and b are the radii of the cranks, and α and β are dependent on their relative inclinations to the above-mentioned vertical plane at starting.

As an example, suppose that $a=b$, while the ratio of m to n is as 2 to 1; then the above equation will represent the curve of pressure for the octave. Similarly, if m is to n as 16 to 15, the resultant

* It should be observed here that the vibratory motion thus given to the frame is not truly harmonic. In order to make it so, a more complicated contrivance than the simple crank and connecting-rod would have to be adopted; but this would probably introduce, through unavoidable play, an error greater than the present one, the length of the connecting-rods and the small size of the cranks rendering the latter nearly inappreciable. The motion will, however, for the sake of convenience, be considered truly harmonic throughout.

curve represents the effect on the ear of a diatonic semitone, while the ratio 81 to 80 would give that of the comma. In both these curves, and more especially in the latter, the beats which would ensue on actually sounding the two tones together are shown with remarkable distinctness.

As the machine is provided with a set of change-wheels, many different curves can be produced, while the form of each can be more or less changed by altering the relative positions of the cranks before bringing the idle wheel into gear. It is also possible to obtain very large values of m and n in the above equation by using two idle wheels on the same axis, which shall come into gear, the upper one with the wheel on the one spindle, the lower one with that on the other.

Thus, suppose A and B are the numbers of teeth on the spindle-wheels respectively, C and D those on the idle wheels, and let A gear with C and D with B; then $\frac{m}{n} = \frac{BC}{AD}$. Now, by properly choosing the four wheels, large values of m and n may be obtained. If, for instance, A=81, B=80, C=55, and D=27, $\frac{m}{n} = \frac{4400}{2187}$; this

ratio being nearly $\frac{2}{1}$, the corresponding curve will represent the effect of an octave slightly out of tune. The period of such curves as these being very long, it is necessary to have a good supply of paper; and this is arranged by carrying a reelful on the horizontal frame, from which it is slowly unwound between the rollers. The rate at which this takes place has a good deal of influence on the form of the resultant curve; the slower it is, the more compressed will the latter appear. Instead of using paper, the curves, provided the periods are short enough, may be drawn on slips of blackened glass, which can be carried along between the tapes connecting the rollers; they can be at once placed in a lantern and thrown on a screen.

The width of contour of any curve depends on the radii of the cranks: these may have any value between 0 and half an inch; and therefore the limit of possible width at any part will be two inches; so also, by altering the radii, a series of curves may be produced corresponding to the consonances of tones not of the same intensities. Since the maximum width of any curve will be double the sum of the radii of the cranks, the paper is cut to a width of two and a half inches, within which all curves which can possibly be drawn will be comprised.

The instrument is constructed by Messrs. Tisley and Spiller, of Brompton Road, to whom some improvement upon the original model is due.

GEOLOGICAL SOCIETY.

[Continued from p. 155.]

November 19, 1873.—Prof. Ramsay, F.R.S., Vice-President,
in the Chair.

The following communications were read:—

1. "Supplemental Note on the Anatomy of *Hypsilophodon Foxii*." By J. W. Hulke, Esq., F.R.S., F.G.S.

The material for this note was a slab from Cowleaze Chine, containing portions of two individuals of *Hypsilophodon Foxii*—one consisting of a skull with a great part of the vertebral column, the other of a portion of the vertebral column. The author described some details of the structure of the skull, and especially the palatal apparatus. The pterygoids, which are not mesially joined, have a stout body, the posterior border of which bears a very large basisphenoidal process; and the left pterygoid retains the root of a strong quadratic process, in front of which the hollow outer border runs out into an ectopterygoid. In front of the pterygoids the palatines are partially visible, also separated by a fissure. Of the eight vertebræ, the last three are firmly anchylosed, and the seventh and eighth form part of the sacrum. They are constricted in the middle; and their transverse processes, which spring from the junction of two vertebræ, are bent backwards, joining the dilated outer end of the transverse processes of the next vertebra, including a large sub-circular loop. The second fragment of a vertebral column, which belonged to a smaller individual, includes the sacrum and several vertebræ. Near the skull the slab contains several very thin bony plates of irregularly polygonal form, regarded by the author as dermal scutes. In connexion with the question of the generic rank of *Hypsilophodon*, the author stated that in *Hypsilophodon* the centra of the sacral vertebræ are cylindroid and rounded below, whilst in *Iguanodon* they are compressed laterally and angulated below.

2. "The Drift-beds of the North-west of England.—Part I. Shells of the Lancashire and Cheshire Low-level Clay and Sands." By T. Mellard Reade, Esq., C.E., F.G.S.

The author commenced by explaining a section in a cutting at Booth-Lane Station, in which most of the beds seen about Liverpool are typically represented. This section shows in ascending order:—1. Pebble-beds of the Trias; 2. shattered rock; 3. compacted red-sand rubble (ground moraine); 4. lowest bed of Boulder-clay (largely composed of red sand); 5. stratified sand, with shell-fragments; 6. bed of fine unctuous clay; 7. brick-clay (with many shells); 8. sand-bed; 9. stratified yellow sand ("Washed Drift sand").

The author next gave a list of the localities in which shells were found, and stated that in all forty-six species had been met with distributed through the clay-beds, those found in the sand-seams being rare and generally fragmentary and rolled. The shells most

commonly found entire are usually of small size, and of a form calculated to resist pressure,—such as *Turritella communis*, *Trophon clathratus*, and *Mangelia turricula*. *Fusus antiquus* and *Buccinum undatum* are generally represented only by worn fragments of the columella; and *Cyprina islandica* is always found in fragments. The author thought that the association of the various species distributed without order through the clays shows that they could not have lived together on the same bottom, but that they must have been to a great extent transported. He contended that the admixture of shells in the Boulder-clay was due to the tendency of the sea to throw up its contents on the beach, whence changing currents and floating ice might again remove them, and to the oscillations of the land bringing all the beds at one time or another within reach of marine erosive action. He maintained that it is in the distribution of land and sea at the period of deposition of the Lancashire deposits, and not in astronomical causes, that we must seek the explanation of the climate of that period, the conditions of which he endeavoured to explain by a consideration of the proportions of the species and the natural habitats of the shells found in the drifts.

3. “Note on a deposit of Middle Pleistocene Gravel near Leyland, Lancashire.” By R. D. Darbishire, Esq., F.G.S.

The bed of gravel, about 40 feet thick, and about 240 feet above the level of the sea, is covered by yellow brick clay, and overlies an untried bed of fine sea-sand. The shells and fragments occur chiefly at the base of the gravel.

The most noticeable shells in this list of forty-two species, collected by Miss M. H. Farington, were *Panopæa norvegica*, *Mastra glauca*, *Cytherea chione*, *Cardium rusticum*, *Fusus propinquus*, and *Fusus antiquus*, var. *contrarius*. One specimen of a *Fusus*, doubtfully identified as *F. Fabricii* (*craticulatus*), had occurred.

The group was by no means characteristically Arctic or Glacial. It represented most nearly the Wexford lists, especially in presenting the reversed *Fusus*, and might be regarded as connecting those beds with the Macclesfield drifts, also containing a Celtic assortment, with *Cytherea chione* and *Cardium rusticum*.

The author considered the Leyland deposit, like those on the west of the Derbyshire hills, to be more probably littoral and truly climatic than that of the Liverpool clays, the subject of Mr. Reade's Paper, and hazarded the conjecture that the latter were sea-bottom beds, into which, during some process of degradation and redistribution, the specimens found and enumerated by Mr. Reade had been carried down from the former more ancient retreating coast-lines.

December 3rd, 1873.—Joseph Prestwich, Esq., F.R.S.,
Vice-President, in the Chair.

The following communications were read:—

1. “Notes on the Structure sometimes developed in Chalk.” By H. George Fordham, Esq., F.G.S.

After referring to Mr. Mortimer's paper on the same subject (see

Q. J. G. S. vol. xxix. p. 417), the author stated that in a pit near Ashwell the "Lower Chalk without flints" exhibits a bed of a concretionary nature, the concretions in which are marked nearly all over with lines. The lines are found only on the concretions and in their immediate neighbourhood. The fossils in the bed are invariably crushed, as if by pressure. The author believes that the striae are due to an incipient crystallization arising from the formation of the concretions; and in support of this view he adduced a specimen of iron pyrites from the chalk of Beachy Head, attached to which is a small portion of very hard striated chalk, and suggested that the crystallization of the pyrites had induced a crystallization in the chalk. He considers, however, that in some places an almost identical structure may be due to slickensides, but only in very broken and faulted beds.

2. "A short description of the Geology of the Eastern Province of the Colony of the Cape of Good Hope." By R. Pinchin, Esq., C.E. Communicated by H. W. Bristow, Esq., F.R.S., F.G.S.

In this paper, which was illustrated by maps and sections, the author gave the results of his observations on the geology of the above region. The two principal sections described were from Cape Saint Francis, across the Great Winterhoek and Langeberg ranges, to the lacustrine Triassic rocks near Jansenville, and from Port Elizabeth to Somerset. The lowest rock in the first section is the quartzite of the Great Winterhoek, which is immediately overlain to the northward by clay-shales and sandstones containing Devonian fossils. Beds with similar fossils occur at the Kromme river, Cape St. Francis, and near Uitenhage. A patch of horizontal secondary strata stretches west from the Gamtoos river, overlying the Enon conglomerate in the same way as the Jurassic strata of Uitenhage. They contain no fossils. The Enon conglomerate is seen on the flanks of the higher hills. The northern ranges, Langeberg, Klein Winterhoek, and Zuurbergen, are regarded by the author as formed of rocks belonging to the Carboniferous series, although closely resembling those of the Great Winterhoek in lithological character, except that among them are bands of the peculiar rock described by Bain as "Claystone porphyry," by Wyley as a "Trap conglomerate," by Tate as a "Trap-breccia," and by Atherstone as an "intrusive Trap." Rubidge regarded it as a metamorphic rock; and this view is adopted by the author, who describes it as underlying and overlying the clay-shales, which always separate it from the quartzite, and as passing imperceptibly into the clay-shales. The mottled sandstone or Ecce rock is referred by the author to the Carboniferous series. The author also noticed the occurrence of Tertiary or recent rocks containing remains of Mollusca identical with species now living in the adjacent seas, lying unconformably upon the Devonian, and conformably upon the Secondary rocks at various places near the coast.

3. "On the Mud-craters and geological structure of the Mekran Coast." By Lieut. A. W. Stiffe, F.R.A.S. Communicated by Prof. Ramsay, F.R.S., V.P.G.S.

The coast of Mekran, extending from near the western frontier of India to the mouth of the Persian Gulf, was stated by the author to be a nearly rainless district, consisting of clay plains with precipitous tabular hills, the former veined here and there with crystalline gypsum, the latter composed of clay capped and sometimes interstratified with coarse, friable, fossiliferous calcareous strata, from 5 to 30 feet thick, supposed to be of Miocene age, and all horizontal or nearly so, except at the extreme east and west, where the strata are inclined at an angle of from 40° to 60° . Along the coast there are no distinct traces of volcanic action; but on the north coast of the Persian Gulf a similar formation has been much disturbed by the protrusion of recent volcanic material, near Jâshak to the west there is a hot mineral spring, and near Karâchi there are springs of pure hot water. The author described the mode in which denudation is effected in this region by occasional heavy rains, and by the constant action of the sea upon the coast, and then noticed the occurrence, within a few miles of the shore, of numerous peculiar mud-craters, forming hills varying in height from 20 to 300 or 400 feet above the plain, of a regular conical form, with truncated tops, and the sides sloping at an angle of about 40° . The summits of these hills present a circular cup with a narrow border, filled with semifluid mud, which occasionally flows slowly over the margin of the crater. The author considered that the conical hills have been formed solely by these overflows. He believed that a small shoal occurring off the coast near Jâshak might be produced by one of these craters, and was inclined to ascribe their existence to hydrostatic pressure rather than to volcanic action, especially as by the concurrent testimony of several natives the discharge from the craters is greater during spring tides. The thickness of the clay forming the plain is probably very considerable; it extends for some miles from the shore, sinking gradually to 20 or 30 fathoms, when there is a sudden and often precipitous descent to a depth of 300 or 400 fathoms. The author suggested that, since the deposition of the Miocene beds, the great submarine cliff may have been raised above the sea, that the land was then depressed to near its present level, causing the removal of the beds to the present coast line, and that a further depression followed by upheaval gave origin to the inland cliffs. Evidence of the last depression is furnished by the presence of borings of lithodamous mollusca in the cliffs considerably above the present sea-level.

XXXIII. *Intelligence and Miscellaneous Articles.*

ON THE LIGHT REFLECTED BY PERMANGANATE OF POTASSIUM.

BY DR. EILHARD WIEDEMANN.

PROFESSOR STOKES* observed that in the spectrum of the light reflected from solid permanganate of potassium dark streaks occur, and that they are exhibited most distinctly with a certain angle of incidence; further, the minima of brightness in the spectrum of the reflected light correspond to the rays transmitted in the greatest intensity by the permanganate.

I have pursued this subject further, and examined not only the light reflected at the boundary of permanganate of potassium and air, but also that at the boundary of benzine, sulphide of carbon, and a mixture of these two substances, and the above salt. Moreover the polarization of the incident light was kept in view. To obtain the reflecting surfaces, triturated crystals of the salt were polished upon ground glass plates by means of a jet-burnisher. Clean surfaces, free from oxide, were thereby secured for the investigation, which is not the case when whole crystals are employed. The glass plate thus prepared was inserted in a rectangular hollow prism (which could be filled with the different liquids) in such wise that its coated face was turned to the rectangular edge. The prism was placed upon a graduated circular table that could be rotated, and sunlight so thrown upon one of the two surfaces including the right angle that the light refracted there fell upon the coated plate and, through reflection, passed out at the other surface. Thence it arrived at the slit of a spectrum-apparatus. The angle of incidence on the coated plate was determined thus: the light from the first surface of the prism was reflected back in its own direction; the position of the table was then read off; the rotation of the table with the prism gives immediately the incidence-angle at the first surface; from this angle and that between the glass plate and the first face of the prism, and the index of refraction of the medium in contact with the permanganate, the incidence-angle at the latter can then be found.

The position of the streaks in the spectrum was determined by means of a photographed scale applied to the spectrum-apparatus, the cross-threads of the observing-telescope having previously been placed on the centre of the dark streak.

These positions with pretty large angles of incidence are given in Table I. The columns under A refer to the streaks in the light polarized parallel to the plane of incidence, those under B to those in the light polarized perpendicular to that plane. The first column gives the names of the surrounding media. Table II. gives the positions of the absorption-streaks in the transmitted light. Fraunhofer's lines correspond as follows to the strokes on the photographed scale:—

$$D=0; E=18; b=21; F=33.$$

* Phil. Mag. 1853, vol. vi. p. 393. Pogg. Ann. 1854, vol. xci. p. 300.

TABLE I.

	A.						
Air	7	14	22	28 $\frac{1}{2}$			
Benzine	7 $\frac{3}{4}$	15	23 $\frac{1}{4}$	30	37		
Mixture of benzine and sulphide of carbon.	8 $\frac{1}{2}$	15 $\frac{1}{2}$	23 $\frac{1}{2}$	31 $\frac{1}{2}$	38 $\frac{1}{2}$	45	
Sulphide of carbon	8 $\frac{1}{2}$	16	24 $\frac{1}{2}$	32	39 $\frac{1}{2}$	47	
	B.						
Air	3 $\frac{1}{2}$	9 $\frac{1}{2}$	16 $\frac{1}{2}$	24	32	38 $\frac{1}{2}$	
Benzine	3 $\frac{1}{2}$	9 $\frac{1}{2}$	16 $\frac{1}{2}$	24	32	38	
Mixture of benzine and sulphide of carbon.	4	9 $\frac{1}{4}$	16 $\frac{1}{2}$	24 $\frac{1}{4}$	32	39	54 $\frac{1}{2}$
Sulphide of carbon	3 $\frac{1}{2}$	9 $\frac{1}{4}$	16 $\frac{1}{2}$	24 $\frac{1}{2}$	32	39 $\frac{1}{2}$	47

TABLE II.

4 $\frac{3}{4}$, 11 $\frac{1}{4}$, 18 $\frac{1}{2}$, 26 $\frac{1}{4}$, 33 $\frac{1}{4}$.

These numbers show :—

1. That, with large angles of incidence, the streaks in the light polarized perpendicular to the incidence-plane, with respect to those in the light polarized parallel to the plane of incidence, are displaced towards the blue, and that in the former another streak occurs in the vicinity of D.

2. That with the increase of the refraction-index of the surrounding medium the streaks in the parallel-polarized light undergo displacements towards the blue; while, on the contrary, in the perpendicularly-polarized light the streaks preserve their position unchanged, or alter it but little. Observation of the streaks in the blue beyond F is attended with great difficulties, as is the entire investigation, through the breadth of the streaks and the impossibility of obtaining perfectly reflecting surfaces.

A comparison of the streaks obtained in the transmitted and in the reflected light shows that never do two of such streaks cover one another, and that neither do the former lie each in the middle between two of the latter.

As to change of position of the streaks with the angle of incidence, it resulted that in the light polarized parallel to the plane of incidence, and likewise in natural light, the position was as good as independent of the angle of incidence; but in the light polarized perpendicular to that plane the streaks have, up to a certain angle of incidence, which amounted to

Air.	Benzine.	Sulphide of carbon.
58 $\frac{1}{2}$ °,	about 52°,	about 52°,

the same position as in the parallel-polarized, and then, with a small alteration of the incidence-angle, suddenly suffer a displacement characterized by the appearance of the streak the details of

which are given in the first column under B. Accordingly, for angles greater than those given, the above Tables hold good.

Precisely the same phenomena as on the ground and polished salt may be observed on crystals. Just so are they exhibited on permanganate of ammonia; but here measurements were impossible, on account of the great decomposability of the salt.

The above observations were verified in every way possible. For example, the dependence of the situation of the streaks on the index of refraction was again established by putting benzine and sulphide of carbon in layers one above another, immersing a glass plate coated with polished permanganate of potassium, and comparing immediately the spectra of the light reflected at the boundaries of the two media by the permanganate. The streaks in the spectrum of the light which had passed through the sulphide of carbon were, in relation to those in the spectrum of that which had traversed the benzine, displaced towards the blue.—Poggendorff's *Annalen*, 1874, No. 4, pp. 625–628.

ON THE TEMPERATURE OF THE SUN. BY J. VIOLE.

I have previously indicated and discussed the method I most frequently employ in my measurements concerning the temperature of the sun. I shall today describe the apparatus I use, and shall develop the calculus of the experiments.

My apparatus is composed of two concentric spherical envelopes of brass. The interior one, 15 centims. in diameter, constitutes the enclosure, in the centre of which is the bulb of the thermometer submitted to experiment. This enclosure, blackened on the inside, is kept at a constant temperature by a continuous current of water furnished by the conduit-pipes of the city and circulating between the two balls. The exterior ball has a diameter of 23 centims.; it has been carefully polished on its outer surface, and is, besides, protected by screens which leave free only the admission-aperture. This aperture is at one of the extremities of a brass tube 17.5 milims. in diameter, directed along one of the radii of the sphere, and opening at the other end into the inner ball. The free extremity of the admission-tube carries a movable diaphragm pierced with three circular apertures of different sizes. Three other tubes traverse, in radial directions, the space comprised between the two spheres: two of them, placed one at 45° , the other at 90° from the admission-tube, serve, the one or the other according to circumstances, to give passage to the stem of the thermometer; the third, closed by ground and slightly blackened plate glass, is directed along the prolongation of the admission-tube, and permits the ascertaining that the solar rays fall exactly on the bulb of the thermometer. The suitable orientation of the apparatus is, besides, attained without difficulty, thanks to its spherical form, which permits it to be turned gradually in the wished-for direction upon a circular ring which serves as its support.

The following is the course of an experiment:—All the tubes being carefully closed, and the thermometer in place, the temperature (which is stationary if all has been well regulated for a sufficient time) is read; then the admission-tube is opened after bringing opposite to it such aperture of the diaphragm as is judged suitable. Now, the apparatus being kept in accurate orientation, we wait until the temperature again becomes stationary, and then note the excess shown by the thermometer.

Experiment shows that this excess depends both on the thermometer employed and on the diameter of the aperture of admission. No precise conclusion, therefore, can be drawn from experiments in which we have not preoccupied ourselves with the dimensions of the thermometer, and with the magnitude of the admission-aperture pierced in the enceinte, with the temperature constant. On the contrary, by employing in succession different thermometers and different apertures of the diaphragm, we can evaluate very accurately:—(1) the cooling due to the contact of the air; (2) the heating which proceeds from the radiation of the portion of the sky bordering the sun and seen at the same time from the bulb of the thermometer. I shall show this by an example, the data of which I take from one of my last series of observations.

On the 20th of June last, operating successively with two thermometers, the spherical reservoirs of which had the diameters 12 millims. and 7 millims. respectively, and with three different apertures *a*, *b*, *c* of the diaphragm, the respective diameters of which were 17·5, 14·5, and 12 millims., I obtained the following results:—

Time.	Temperature of the enceinte.	Temperature of the large thermometer.	Temperature of the small thermometer.
h m			
2 40	14°10	27°03 (diaphragm <i>a</i>)	
2 55	14°05	26°56 (diaphragm <i>b</i>)	
3 10	14°05	24°05 (diaphragm <i>b</i>)
3 30	14°00	23°63 (diaphragm <i>c</i>)
3 45	13°95	23°85 (diaphragm <i>a</i>)
4 10	13°90	26°05 (diaphragm <i>a</i>)	
4 20	13°85	23°43 (diaphragm <i>a</i>)
4 35	13°80	23°30 (diaphragm <i>a</i>)

Let us take first the observations of 2^h 55^m and 3^h 10^m; these two, made nearly at the same time, should lead to sensibly equal excesses of temperature. The considerable difference between the two numbers observed arises from the complication introduced into the experiment by the presence of air: to the radiation from the bulb of the thermometer is added the cooling produced by the air; and in these two ways the bulb loses a quantity of heat equal to that which it receives from the sun. The loss of heat *in vacuo*, making equilibrium with the same quantity of heat received from the sun, would therefore be equal to the loss observed plus the loss due to the air. But, according to Dulong and Petit, the lowering

of temperature resulting from this last cause can be represented by $\frac{m}{R} \tau^{1.233}$, m being a constant dependent only on the elasticity of the air, and τ the observed excess. We should have, then, *in vacuo* with the two thermometers the two equal excesses

$$12.51 + \frac{m}{6} 12.51^{1.233} = 10 + \frac{m}{3.5} 10^{1.233}, \text{ whence } m = 2.24.$$

Let us take in the same way the observations of 3^h 45^m, 4^h, and 4^h 20^m, all three made with the same diaphragm, but different from the preceding one; they conduct to the equation

$$9.77 + \frac{m}{3.5} 9.77^{1.233} = 12.15 + \frac{m}{6} 12.15^{1.233}, \text{ whence } m = 2.09.$$

Let us adopt for the value of the coefficient of cooling m the mean of the two values thus obtained, $m = 2.15$; with the aid of this coefficient we can draw up the following Table of the temperatures which would have been observed *in vacuo* :—

Time.	Temperature of the enceinte.	Temperature of the large thermometer.	Temperature of the small thermometer.
h m			
2 40	14° 10	35° 40 (diaphragm <i>a</i>)	
2 55	14° 05	34° 64 (diaphragm <i>b</i>)	
3 10	14° 05	34° 54 (diaphragm <i>b</i>)
3 30	14° 00	33° 65 (diaphragm <i>c</i>)
3 45	13° 95	34° 20 (diaphragm <i>a</i>)
4 0	13° 90	33° 83 (diaphragm <i>a</i>)	
4 20	13° 85	33° 50 (diaphragm <i>a</i>)
4 35	13° 80	33° 10 (diaphragm <i>a</i>)

On tracing the curve representing the course of the thermometer for one and the same admission-aperture, it is readily recognized that the relative temperatures at the different periods all combine with perfect regularity, whether they come from the large or the small thermometer.

Let us now consider two experiments made with different diaphragms; and as the small thermometer is that which approximates most nearly to the theoretical conditions (especially for small admission-apertures), let us take the three experiments relative to 3^h 10^m, 3^h 30^m, and 3^h 45^m. Making use of the curve of the temperatures for the diaphragm *a*, and reducing all to one and the same temperature, 14°, of the enceinte, we have for the temperatures at one and the same period:—

Diaphragm <i>a</i>	34° 45
Diaphragm <i>b</i>	34° 08
Diaphragm <i>c</i>	33° 70

Applying to these data the equation I established in my previous

note,

$$Sa^{\theta} = Sa + \omega a^x + \Omega a^y \quad \text{or} \quad a^{\theta} - a^t = \frac{\omega}{S} a^x + \frac{\Omega}{S} a^y,$$

we have

$$\begin{aligned} (\text{Diaph. } a) \quad 1.0077^{34.45} - 1.0077^{14} &= \frac{1}{183960} 1.0077^x \\ &+ \left(0.0009493 - \frac{1}{183960} \right) 1.0077^y, \end{aligned}$$

$$\begin{aligned} (\text{Diaph. } b.) \quad 1.0077^{34.08} - 1.0077^{14} &= \frac{1}{183960} 1.0077^x \\ &+ \left(0.0006516 - \frac{1}{183960} \right) 1.0077^y, \end{aligned}$$

whence $x = 1355^{\circ}$, and

$$\begin{aligned} (\text{Diaph. } a) \quad 1.0077^{34.45} - 1.0077^{14} &= \frac{1}{183960} 1.0077^x \\ &+ \left(0.0009493 - \frac{1}{183960} \right) 1.0077^y, \end{aligned}$$

$$\begin{aligned} (\text{Diaph. } c) \quad 1.0077^{33.7} - 1.0077^{14} &= \frac{1}{183960} 1.0077^x \\ &+ \left(0.0004465 - \frac{1}{183960} \right) 1.0077^y, \end{aligned}$$

whence $x = 1353^{\circ}$.

The agreement of the two values of x shows that the correction necessitated by the radiation of the region of the sky in the vicinity of the sun is made with sufficient exactness by writing for the total radiation of the different portions of this surface $\frac{\Omega}{S} a^y$, as if all these parts were at one and the same mean temperature y .

Therefore, in the example selected, it will be concluded from these calculations that, on the 20th June, at Grenoble, the temperature of the sun, defined as I have indicated above, was, at 3^h 30^m,

$$1354^{\circ}.$$

But this number itself, to give the *true* temperature of the sun, ought to be further corrected on account of divers influences, particularly the absorption of the terrestrial atmosphere. It is chiefly by operating at different altitudes, and (of course) noting the pressure and the hygrometric state of the air at each station, that I hope to solve this problem. For this purpose I have already made several ascents of the Alps; and I shall resume them as soon as possible.—*Comptes Rendus de l'Acad. des Sciences*, June 29, 1874.

PHYSICS OF THE INTERNAL EARTH.

BY D. VAUGHAN.

In 1853 I first attempted to trace the consequences of subterranean heat, by taking into consideration some facts and principles which seemed to have received but little attention. The results of my inquiries on the subject were given in a circular in 1854, in a pamphlet in 1856, and in a paper which I sent to the British Association in 1861, and of which an abstract is published in the Reports of the Sections, page 134. In that paper I endeavoured to show that the terrestrial crust, if reposing on lava of a declining temperature, would receive accessions of buoyant solid material, chiefly on such points as extend deep into the fiery menstruum, and that the consequent growth of internal mountains would be interrupted only by the occasional movements of vast portions of this light matter to positions much higher than those at which they were first deposited. To the collisions of such rising masses against the weaker parts of the earth's crust I ascribe earthquakes; but the theory affords a more satisfactory explanation for volcanic phenomena.

Avalanches of siliceous rocks, ascending through buoyancy from deep subterranean peaks or depressions, would lead to important results by conveying heat from a lower to a higher stratum of the internal earth. Owing their solidity to pressure, such stony masses would fuse during the ascent; and, like our mountain-floods, would erode channels which must for a long period direct them to the same localities. The same spots of the earth's crust, being thus exposed for many ages to the repeated inroads of intensely heated matter from great depths, would be reduced in thickness by the frequent fusion, and would present a weaker barrier to subterranean violence. Such an internal convection of heat would end in perforating the earth's crust and producing an immense lake of lava on its surface, were it not for the cooling influence of aqueous action; and the presence of water on our globe, though tending much to increase the violence of earthquakes and volcanic eruptions, has the effect of confining their ravages within a more limited range. To the absence of water from the moon we may ascribe the enormous diameters of the craters of lunar volcanoes; while their height is displayed on a far less scale, and there are no long ranges of lunar mountains. On our satellite also volcanoes have for the most part an insular character, conforming little to the linear arrangement so common on the earth; so that the cooling agency of water appears to have been concerned in producing the vast rents or fissures on which so many volcanic orifices seem to be located.

Apart from the evidence which the pendulum and geodetic measurements give of inequalities on the invisible side of the earth's crust, it can be proved theoretically that they are inevitable in the course of solidification over the molten mass. One source of solid matter light enough to form the external framework of our globe

is to be found in the decomposition of many of the heavy silicates by enormous pressure when the temperature of the menstruum in which they were fused sunk below the melting-point of quartz. An equivalent of oxide of lead and of crystallized silicic acid would have their common volume increased about 14 per cent. on combining and forming lead glass. Now, at a depth of 1000 kilometres below the earth's surface, the pressure is equal to about 300,000 atmospheres; and accordingly the formation of a cubic inch of glass by the union of quartz and oxide of lead would, in consequence of the expansion it involves, be resisted by a force the thermal equivalent of which may be represented by the heat expended in melting 14 cubic inches of ice. A force of equal energy would be exerted by the same pressure for the decomposition of a cubic inch of silicate of lead, in the supposed locality, and for the crystallization of the resulting silicic acid. As far less heat is evolved by the union of the strongest acids and bases, and as a crystallization or atom-arrangement can make no heavy demands on force, it is reasonable to conclude that in the supposed case chemical affinity would be overruled and that the silicate of lead would be decomposed. From similar estimates it would also appear that other silicates, especially those of heavy metals, would undergo a similar decomposition at great depths, and would part with their silica when the temperature became low enough to allow its solidification.

Another source of buoyant matter is to be found in the transfer of silica from the heavy metallic oxides to the alkalies and other strong bases. The light compounds thus formed would, according to Delesse and Deville, contract more than other igneous rocks in passing into a solid state; and it is evident that in proportion to this contraction will their production be favoured by pressure on the decline of the primitive heat. The growth of a floating crust would also be promoted by other circumstances. Of many of the metallic oxides, the most infusible compounds are those in which the silicic acid is very small or in a very large proportion. But the latter bodies, which have almost invariably the lowest specific gravity, have also their fusibility reduced most by pressure in consequence of the contraction which they undergo in assuming a solid form. On this point more satisfactory evidence may be obtained by an investigation similar to that of Clausius, but in which the effects of pressure upon fusion is determined from the change of volume and the modulus of elasticity.

Of the various products which separate from the subterranean lava in cooling, the most dense parts would sink to the centre, though solidifying in the uppermost stratum; while the lighter material, though taking the solid form at great depths, would rise towards the surface. But the solidity of the light silicated matter could be permanent only when kept under the influence of immense pressure, by settling on prominent points which extend from the inner side of the crust deep into the lava. The great centres of accumulation of this buoyant matter must be under continents, where

the earth's crust has evidently the greatest thickness and reposes on the deepest internal prominences ; and to the occasional slides and ascending movements of matter from these parts of the subterranean regions, we may ascribe the prevalence of vulcanicity on so many continental coasts.

If only one per cent. of terrestrial matter passed into a solid form in the course of ten millions of years, there would be still sufficient grounds for assigning to rock-slides a mass so great that the mechanical effects of their collisions against the thinner parts of the crust may produce the most violent earthquake shocks. But the most obvious effects must be ascribed to the sudden elevation of temperature which the thin spots of the earth's crust should experience, and which may be reasonably estimated at many thousand degrees. Exposed to such a fierce heat, the solid structure would be rent by the unequal expansion of its parts, or by the elasticity of its volatile constituents. Steam would manifest an irresistible power when rock containing moisture tumbled into the molten liquid or encountered it when penetrating through fissures. But a motive power of long continuance would arise from the property which silica has of expelling other acids from bases at high temperature. As the siliceous rocks come into collision with the strata containing limestone or any other carbonates, the resulting mass should swell with the evolution of carbonic acid, and boil over a volcanic crater or even open a new one. In consequence of the pressure, this expulsion of carbonic acid will require a higher temperature ; and the cooling, chiefly through the agency of water, would soon occasion a state of repose until there occurred a new influx of heated matter from deep regions. An estimate of the rate of cooling, as involved in the mere production of steam alone, would show that, during their numerous eruptions, Etna and Vesuvius must have lost a quantity of heat too great to be supplied by any conceivable chemical or mechanical action in their immediate vicinity ; and evidence may be thus obtained of the necessity of the convection of caloric, and of the introduction of incandescent matter from distant localities to the theatre of volcanic activity.

Cincinnati, O., July 15, 1874.

ON THE CONVERSION OF ORDINARY INTO AMORPHOUS PHOSPHORUS BY THE ACTION OF ELECTRICITY.

In the *Anzeiger* of the Imperial Academy at Vienna, Professor v. Schrötter gives the following notice of this transformation, discovered by Dr. Geissler :—

Already in 1860 Dr. Geissler endeavoured to show that electricity by itself effects this change ; and he had the goodness, on the occasion of his visit to Vienna at the time of the Universal Exposition, to give up to me some of the glass apparatus.

The simplest of these is an exhausted glass tube of about 35 cen-

tims. length and 2 centims. diameter, to the ends of which additions were attached (by fusion) containing the conducting-wires, so that in the experiment the wires were at least 45 centims. distant from each other. The tube was filled with phosphorus vapour of very little tension; after the experiment its sides were coated with a thin layer of amorphous phosphorus brownish red changing to golden yellow, and in many places exhibiting the colours of thin films.

The second apparatus serving for the same purpose, a masterpiece of the glassblower's art, has the form and size of a beaker-shaped double-walled champagne-glass. The thin layer of amorphous phosphorus distributed over the inner surfaces of its walls exhibits the play of all the colours of thin films, giving to the glass a pleasing appearance.

The third, still more elaborately executed apparatus is designed to show that the conversion of the phosphorus is effected even by the inducing action of the current. For this purpose the ends of the two aluminium conducting-wires are inserted in exhausted spheres in which there is no phosphorus. These spheres are enclosed in others, which are united by a tube 40 millims. long and 1 millim. wide. The interspaces thus formed, likewise exhausted, contain the phosphorus, which is therefore completely shut off from the conducting-wires by a wall of glass. The distance between the conducting-wires amounts to 26, and the diameter of the outer spheres to 5 centims. The interval between the walls of the spheres amounts to 5 millims. Here also the inner side of the outer, and the outer side of the inner sphere, in like manner as above stated, were coated with amorphous phosphorus. Only in the narrow connexions was no phosphorus deposited.

The above-mentioned facts furnish, perhaps, the best demonstration that the conversion of phosphorus into the amorphous modification is effected neither by the light nor by the heat which accompany the current, but exclusively by the electricity itself.

The instructive experiments which Hittorf published in 1865 (*Pogg. Ann.* vol. cxxvi. p. 195) were made with another arrangement of the apparatus, as the platinum wires, fused into glass spheres of 6 to 8 centims. diameter, were only a few millims. distant from one another; so that sparks passed, and the course of the phenomenon was somewhat different from that above described; but the conclusions deduced therefrom by Hittorf were the same.

I hope to be able to resume this subject in greater detail; for the present the above account may suffice to recall attention to it. —*Poggendorff's Annalen*, vol. clii. pp. 171–173.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

OCTOBER 1874.

XXXIV. *On Gladstone's Experiments relating to Chemical Mass.*
By EDMUND J. MILLS, D.Sc., F.R.S.*

I. **T**HE Philosophical Transactions for 1855 (vol. cxlv.) contains an important memoir by Gladstone "On Circumstances modifying the Action of Chemical Affinity." In this memoir numerous sets of experiments are described, which mainly serve to determine, by means of an increase or diminution of colour, the progress of certain selected reactions. The results are exhibited in curves, several of which show a regular course, while all are continuous; but no mathematical expression of the law of action is given. About eleven years afterwards (Phil. Trans. 1865-6) it was shown by Esson, on the basis of Harcourt's experiments, that when a substance undergoes chemical change, the residue y of changing substance is connected with the unit intervals x of change (time, reagent, or other operator) by the equation

$$y = a\epsilon^{-\alpha x},$$

where a represents the amount of substance originally present, and α the amount of it disappearing per unit of x . This relation is graphically represented as a logarithmic curve; but, as a rule, even in very simple cases, its expression is more complex, and corresponds to the form

$$y = a_1\epsilon^{-\alpha_1 x} \pm a_2\epsilon^{-\alpha_2 x},$$

which indicates that two bodies are undergoing change, or that one body is undergoing dual change. In either case the amount

* Communicated by the Author.

of change per unit interval is proportional to the amount of substance then changing.

As Gladstone's results were the first in which the continuity of the chemical process was experimentally demonstrated (at any rate on a sufficient scale), I felt much interested in ascertaining whether Esson's equation would apply to them—especially when I considered how few have been the contributions to chemical dynamics, the laborious (and consequently unpopular) nature of such researches, and the inexpediency of allowing good work to remain dumb or unexpressed.

II. The colorimetrical method, which was used throughout by Gladstone, has considerable disadvantages, and is most serviceable when only small quantities, as in the case of the Nessler test, have to be measured and an inaccuracy of about 5 per cent. is of no consequence. It is probable that the observer's estimate of colour varies during a long course of experiments, and is really under training in the earlier ones; so that, as will actually be found below, all the more serious errors occur, as a rule, at the outset. We must also remember that colour-effects in solutions are not unfrequently slow in attaining their maximum, thus making a particular observation too low; on the other hand, the subsequent arrival of this maximum will make a following observation too high: hence also, by virtue of compensation, the later observations may be expected to be more correct.

A further difficulty lies in the computation itself. The amount of chemical energy (or substance) originally present is not given in terms of the reagent, and has to be arrived at by successive and wearisome approximations; and these might perhaps have been carried a stage further with advantage. Again, the successive values of x are very seldom given in the experiments, which had not been arranged to test any particular hypothesis; they had consequently to be obtained by graphic interpolation on curves which, for such a purpose, should have been considerably longer.

If we bear in mind these and other drawbacks, we shall regard the coincidence between theory and experiment as very striking.

III. *Ferric Nitrate and Potassic Sulphocyanide* (*loc. cit.* p. 187, pl. 7. fig. 1).—To one "equivalent" of ferric nitrate successive groups of "equivalents" of potassic sulphocyanide are added, in presence of water; the amount of "red salt produced" is estimated by eye-observations, the liquid being diluted for that purpose up to a standard. The total amount a of red salt thus producible represents in special measure the original unexhausted energy of the nitrate. I have taken each unit of x as representing 25 "equivalents" of potassic sulphocyanide. The equation

is

$$y = 401(\cdot 8550)^x + 224(\cdot 1516)^x;$$

but the results are expressed in percentages of the initial value of y .

TABLE I.

x .	y , calculated.	y , found.
1	60·3	60·2
2	47·7	47·7
3	40·2	39·2
4	34·3	33·0
5	29·3	28·5
6	25·1	24·5
7	21·4	20·6
8	18·3	17·8
9	15·7	15·4
10	13·4	13·3
11	11·5	11·7
12	9·8	10·4
13	8·4	8·8
14	7·1	7·2
15	6·1	6·1

Gladstone gives two variations of this experiment.

IV. *Ferric Sulphate and Potassic Sulphocyanide* (*loc. cit.* p. 189, pl. 7. fig. 1).—On account of the weakness of the colour produced when a sulphate is present, the amount of the salts employed was doubled. One equivalent of ferric nitrate was taken. The equation is

$$y = 438(\cdot 8208)^x + 132(\cdot 1400)^x;$$

and the unit of x is 15 equivalents.

TABLE II.

x .	y , calculated.	y , found.
1	66·3	65·3
2	52·2	53·9
3	42·6	44·2
4	34·9	35·6
5	28·6	28·7
6	23·5	23·7
7	19·3	19·6
8	15·8	15·3
9	13·0	12·0
10	10·7	9·6
11	8·7	8·1
12	7·2	6·7
13	5·9	5·6

V. *Ferric Chloride and Potassic Sulphocyanide* (*loc. cit.* p. 189, pl. 7. fig. 1).—This experiment is described as “precisely analogous to the preceding.” The equation is

$$y = 406 (\cdot 8900)^x + 214 (\cdot 2500)^x;$$

and the unit of x is 20 equivalents.

TABLE III.

x .	y , calculated.	y , found.
1	66·9	65·0
2	54·0	54·0
3	46·7	46·9
4	41·2	41·2
5	36·6	36·8
6	32·6	33·2
7	29·0	29·5
8	25·8	25·8
9	22·9	22·9
10	20·4	20·0
11	18·2	17·3
12	16·2	14·8

VI. *Ferric Nitrate and Hydric Sulphocyanide* (*loc. cit.* p. 190, pl. 8. fig. 2).—Ferric nitrate, 1 equivalent. Unit of $x = 4$ equivalents. The equation is

$$y = 535\cdot 5 (\cdot 91093)^x + 89\cdot 5 (\cdot 32670)^x.$$

TABLE IV.

x .	y , calculated.	y , found.
1	82·7	82·7
2	72·6	73·1
3	65·3	65·3
4	59·1	58·9
5	53·8	53·9
6	49·0	49·6
7	44·6	45·3
8	40·6	41·3
9	37·0	37·6
10	33·7	34·4
11	30·7	31·2

The above results seem to have been the sequel of considerable experience with the method, and are in exceptional accordance with theory.

VII. *Ferric Citrate and Hydric Gallate* (*loc. cit.* p. 193, pl. 9. fig. 1).—One equivalent of ferric citrate was mixed with 5 &c. equivalents of hydric gallate, and the increasing black coloration measured. Unit of $x = 3$ equivalents. The equation is

$$y = 660 (\cdot 9127)^x + 90 (\cdot 3072)^x.$$

TABLE V.

x .	y , calculated.	y , found.
1	84.0	86.7
2	74.4	76.0
3	67.2	68.4
4	61.2	62.1
5	55.8	56.3
6	50.9	50.8
7	46.4	46.0
8	42.4	41.7
9	38.7	38.0
10	35.3	34.7
11	32.2	32.1

VIII. *Ferric Citrate and Potassic Ferrocyanide* (*loc. cit.* p. 199, pl. 9, fig. 5).—One equivalent of ferric citrate was mixed with 3 &c. equivalents of potassic ferrocyanide in presence of hydric oxalate, and the increasing blue coloration determined. Unit of $x = 3$ equivalents. The equation is

$$y = 102 (\cdot 2010)^x + 23 (\cdot 7699)^x.$$

TABLE VI.

x .	y , calculated.	y , found.
1	30.6	29.6
2	14.2	14.4
3	9.1	9.6
4	6.6	6.4
5	5.0	4.0

IX. The above equations represent the greater part of Gladstone's results as figured at the end of his memoir. I have not worked out the remainder, either (1) because they form mere continuations or repetitions of the reduced curves, or (2) because the experiments were not numerous enough, nor the theory of the reactions sufficiently evident, to enable the calculation to be made. The curves representing the formation of ferric meconate and acetate somewhat resemble, but are not identical with, the cubical parabola. Similar ones are drawn by Harcourt and Esson (*Phil. Trans.* 1866, pl. 17), and Guldberg and Waage (*Etudes sur les Affinités chimiques*, Christiania, 1867, pls. 14, 15, 16). It is obvious that they represent duplex reactions; but their complete reduction may perhaps be a matter of considerable difficulty.

In order to estimate the accuracy of the experimental work, and the soundness of the hypothesis involved in its symbolic expression, I have drawn up the following error Table, showing

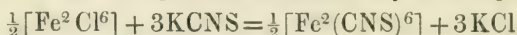
a summary of the differences between calculation and observation, as compared in percentages.

TABLE VII.

	Above 2-3.	Above 1-2 (inclusive).	Above 0.5-1.0 (inclusive).	Above 0.0-0.5 (inclusive).
Table I.	0	1	5	9
Table II. ...	0	3	4	6
Table III. ...	0	2	2	8
Table IV. ...	0	0	5	6
Table V. ...	1	2	4	4
Table VI. ...	0	0	2	3
Totals	1	8	22	36

The entire number of comparisons is sixty-seven. Thus it appears that 5.4 per cent. of the errors are such as would, on their average, be found in very good analytical work; 33 per cent. of them occur, on their average, in ordinarily good analytical work; the remaining 13 per cent. lie, on their average, within the usual limits allowable in colorimetry.

X. The foregoing equations show that any such expression as



is wholly erroneous, if intended to represent the chemical energy of a ferric salt, or the amount of potassic sulphocyanide that is capable of acting thereon; for the energy of the quantity $\frac{1}{2}[\text{Fe}^2\text{Cl}^6]$ is not exhausted until about 400 units (KCNS) have been brought to bear upon it; and other ferric salts are represented by similarly high numbers. The ordinary equations of chemistry represent the result of distributing weight, and give no account of work done; these, on the other hand, represent a dynamical *process* as well as distribution of weight. Hence it is clear that the "equivalents" or valencies inferred from the common equations rest upon a wholly fallacious basis, and cannot be depended upon in scientific reasoning. To assert, for instance, that C is equivalent to H^4 , amounts to stating that hydrogen and carbon have been compared as to the work they can do under certain circumstances, just as ferric chloride is compared with ferric sulphate in Gladstone's experiments. No such research has, however, been made; and it would not be likely to yield the ratio $\frac{\text{C}}{\text{H}^4} = 1$ if it were made. What, then, becomes of the doctrine that carbon is tetravalent?

It is worthy of remark that, while the ordinary equations invariably express that quantity consists of parts (that, for example, potassic chloride contains potassium and chlorine, whereas we

only know that it contains the joint *weights* of potassium and chlorine), the logarithmic equations make no suggestion upon this subject. All the above experiments might have been accurately performed and symbolically expressed by a person totally ignorant of the "constitution" of ferric salts or of potassic sulphocyanide; and the reagent might have been extremely impure, provided that it produced a red coloration. What we owe to Esson and Gladstone we might have inherited from Wenzel or Cavendish.

12 Pemberton Terrace,
St. John's Park. N.

XXXV. *On a very singular Sulphuretted Nitrogenous Compound, obtained by the Action of Sulphide of Ammonium on the Hydrate of Chloral.* By EDMUND W. DAVY, A.M., M.D., M.R.I.A., Professor of Forensic Medicine, Royal College of Surgeons, Ireland, and late Professor of Agricultural Chemistry, Royal Dublin Society*.

THE substance termed hydrate of chloral, or chloral hydrate, from the many valuable therapeutic properties it has recently been found to possess, has within the last four or five years been prepared in considerable quantities, and has become an article of some commercial importance; and numerous as are the useful applications which have already been made of that substance in medicine, there can be but little doubt that their number may be greatly increased; so that we may justly regard chloral hydrate as one of the most, if not the most, important of the recent additions to our materia medica.

It being thus a substance of such practical importance, any information which may tend to extend our knowledge of its chemical properties and relations should not, I conceive, be regarded as devoid of interest. I shall therefore briefly state the results of some observations which I have recently made as to the action of sulphide of ammonium on that substance (a subject that has been but little studied), and describe the properties of a very singular compound thereby produced, the constitution of which, as far as I am aware, has not hitherto been determined.

When sulphide of ammonium is added to an aqueous solution of chloral hydrate, the mixture after a few moments acquires a deep yellow colour, and, rapidly becoming orange, passes to a reddish brown, which finally assumes so dark an appearance that the liquid, when in any quantity, looks almost black by reflected light. It was also observed, after the mixture had

* Communicated by the Author.

assumed an orange tint, that almost immediately more or less of a solid matter invariably separated from the liquid, appearing at first of a bright orange or light red colour, from its being suspended in the orange or red liquid, but that, after it was separated from it by filtration and washing, it was found to possess a light brown appearance. Whilst the changes just described were taking place, it was also noticed that the mixture became sensibly warm to the hand, and that the odour of the sulphide disappeared, whilst that of ammonia and of chloroform was easily detected.

It was further ascertained that when the dark reddish-brown liquid obtained in the way just stated was acidified with an acid, it yielded a copious brown precipitate, which, though somewhat darker in its colour than that which separates from the liquid before the addition of the acid, appears to be essentially the same compound, the difference of shade being probably due, at least in some measure, to different amounts of free sulphur present in each.

As the principal feature of interest connected with the reaction referred to, I considered, was attached to the formation of the brown solid compound just noticed, a quantity of it was made as follows:—Four hundred grains of chloral hydrate being dissolved in about ten ounces of distilled water, sulphuretted hydrogen was passed through the solution till it possessed, after being shaken, the odour of that gas. Sulphide of ammonium was then added in small portions at a time, continuing the passage of the sulphuretted hydrogen through the mixture, when the effects before described were produced. This treatment was continued till no further action appeared to take place, and the mixture possessed, after being well shaken, a strong odour of sulphuretted hydrogen.

I may here observe that, after the addition of the sulphide of ammonium, the evolution of ammonia was from the first perceptible, whilst the odour of the sulphide and of the gas for some time continually disappeared, and it was not till the later stages of the process that the smell of chloroform could be detected.

To the mixture so treated, which was distinctly alkaline, pure diluted sulphuric acid was added till it acquired an acid reaction, and the whole was thrown on a filter, when the brown solid was separated from a deep amber-coloured liquid. The former was then washed with cold distilled water till no indication of sulphuric acid in the filtrate could be detected by chloride of barium; but finding that it exhibited traces of ammonia when treated with caustic lime, the washing of the brown solid was continued, first using cold distilled water; and this failing to accomplish the object sought, it was washed with a considerable quantity of hot

distilled water till the presence of ammonia could no longer be discovered in the filtrate. The brown matter was subsequently dried, first by exposure to the air on the filter at the ordinary temperature, then at a very gentle heat, and afterwards by exposing it for some time under a bell-glass to the drying influence of sulphuric acid.

As I thought it more than probable that the substance, from the way in which it had been procured, contained some free sulphur (which was afterwards shown to be the case), a portion of that which had been so dried was placed in a stoppered bottle and digested for some days along with bisulphide of carbon; the mixture was then thrown on a filter, and washed with repeated fresh portions of pure bisulphide till but a faint trace of residue remained after the evaporation of a little of the filtrate; and this seemed to be due, not to sulphur as at the first, but to the brown compound being soluble to a very slight degree in the bisulphide. After this treatment the bisulphide was allowed to evaporate off from the substance, when it was placed as before under a bell-glass along with a vessel containing sulphuric acid, where it remained for some days. Thinking, however, that it might still not be perfectly dry, it was subsequently heated in a water-bath or oven to about 212° F., when I found that a very slight amount of moisture was expelled from it, accompanied by a peculiar sulphurous smell; and as soon as it appeared to lose no further weight by this temperature, it was placed in a well-stoppered bottle and reserved for examination.

The substance so obtained, and in this dry condition, possesses the following properties: it is an amorphous solid of a light brown earthy appearance, is easily reducible to a state of impalpable powder, and has a specific gravity of about 1.62. When gently heated on platinum-foil it evolves a very peculiar odour, then blackens, partially fuses, and, taking fire, burns with a purplish-coloured flame, emitting a faint odour of sulphurous acid, whilst it leaves a large carbonaceous residue, which on the application of a stronger heat ignites and slowly burns away.

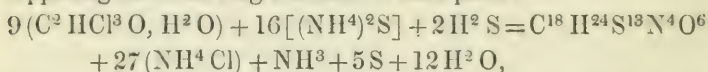
It is very slightly soluble in water, alcohol, bisulphide of carbon, and in ether, whilst it is almost insoluble in chloroform and in benzol. It is, however, readily dissolved by solutions of the caustic alkalies, and by those of the alkaline carbonates and sulphides, forming dark brown or reddish-brown solutions, from which it is again precipitated, apparently unchanged, by the addition of an acid in excess. It dissolves also in solutions of the hydrate of lime and of baryta, and is soluble to some extent in alkaline chlorides and iodides.

As to the action of acids, when it was treated with concentrated sulphuric acid it acquired a darker colour, and dissolved,

forming a brown solution, which on being heated became almost black in appearance; and this on the addition of water gave a flocculent dark brown precipitate resembling the original substance, except in its being of a darker colour.

Strong nitric acid, even at the ordinary temperature, was found to act rapidly on the substance, which it oxidizes and dissolves; but neither it nor sulphuric acid in a diluted condition appears to exercise any effect on it; for when boiled for some time with them no apparent change was observed to take place. As to hydrochloric acid, even when in a tolerably concentrated condition it seemed not to produce any effect on the substance either at the ordinary temperature or when boiled with it.

The compound, some of the properties of which have just been noticed, on being submitted to analysis gave results which agree most closely with the formula $C^{18} H^{24} S^{13} N^4 O^6$, showing that the substance is an extremely complex one, the formation of which, under the circumstances described, may be explained by supposing the following reaction to take place:—



where 9 equivalents of chloral hydrate, being acted on by the conjoint action of 16 of sulphide of ammonium and 2 of hydro-sulphuric acid, give rise to the formation of 1 equivalent of the brown compound, together with 27 of chloride of ammonium, 1 of ammonia, 5 of sulphur, and 12 of water, 9 of which latter exist already as constituents of the chloral hydrate; and the probability that such changes do take place appears to be strengthened by the fact that chloride of ammonium, ammonia, and free sulphur were detected amongst the products of the reaction; and the presence of a trace of chloroform may be easily accounted for by the action of the free ammonia on a portion of the unchanged chloral hydrate.

I may observe that those results as to the composition of the brown compound were obtained as follows:—The carbon and hydrogen were determined by combustion with chromate of lead, using a long combustion-tube and placing a layer of copper turnings in its anterior part; the nitrogen by burning with soda-lime, and estimating the resulting ammonia by means of the chloride of platinum; the sulphur by converting it into sulphuric acid, which was effected by treating the substance with nitric acid and chlorate of potash (as recommended lately by Pearson for the determination of sulphur in organic compounds), and then estimating the sulphuric acid so produced in the usual way by chloride of barium; and lastly the oxygen was determined by difference after the estimation of the other constituents.

But I may remark that the peculiar properties and great complexity of this compound offer considerable difficulties in the way of an exact determination of its different constituents, and of its true nature as a chemical combination. It appears, however, from the circumstance that it readily dissolves in alkaline solutions, which then yield insoluble or sparingly soluble dark-coloured precipitates with different metallic salts, that it partakes somewhat of the character of an acid; but this and several other obvious matters of inquiry connected with the compound are subjects for further investigation.

Before concluding, it is right to state that, after I had observed many of the facts which I have here described, I found, on looking over the 'Chemical News,' that there was in volume xxv. page 37, a notice of a communication "On the Reaction of Chloral Hydrate and Sulphide of Ammonium," which had been read by Dr. J. Walz before the Lyceum of Natural History of New York, in which he notices some of the changes which I have described as taking place in that reaction, as well as the formation of a light-yellow substance, the properties of which (as observed by him) do not altogether agree with those of the sulphuretted compound, which I prepared in a somewhat different manner from that which he adopted. I may also add that Dr. Walz did not attempt to analyze the substance he obtained, for want, as he says, of material—and that he further states, in speaking of it, that O. Löw asserts that in physical appearance and chemical properties it resembles exactly the sesquisulphide of carbon which he has described in the *American Journal of Science*, vol. xli. p. 251.

Be this as it may as regards the substance obtained by Dr. Walz, my analyses of the brown sulphuretted compound, prepared in the manner stated, show that it possesses a totally different chemical composition from the sulphide described by Löw in the *Journal* to which he has referred.

XXXVI. *On Unilateral Conductivity.*

By ARTHUR SCHUSTER, *Ph.D.**

I. *Introductory.*

WHILE I was engaged in other work I met with an irregularity which seemed to me to be of such a peculiar nature that I subjected it to a separate investigation. The results of this investigation have not been entirely satisfactory. I have not been able to raise the phenomenon, to which I allude,

* Communicated by the Author, having been read in Section A. of the British Association at Belfast (1874).

above the rank of an irregularity; that is to say, I am not able to produce it at my own will, although when it is present I am generally able to destroy it. My experiments, however, leave no doubt as to the facts, and they show clearly that, in a circuit composed entirely of copper wires, joined together by means of binding-screws, the electric conductivity may be different in opposite directions. It would be difficult to discover such a difference in the resistance by means of the ordinary ways of measuring it. The changes in the electromotive force of the battery and in the resistance of the wire, through an alteration of temperature or other accidental causes, would be sufficient to mask the effect. If we use, however, the electromotive force of a moving magnet, we are sure that it is always constant as long as the strength of the magnet does not vary and the magnet moves always between certain limits. A magnet rotating rapidly within a coil of wires induces currents in alternate directions in the coil. We are perfectly sure that the electromotive force producing these currents is the same in both directions; and if we can detect any difference in the strength of the currents going in opposite directions through the wire, we may be sure that only a difference in the resistance can produce such a result. I have calculated the effect on the galvanometer-needle of induction-shocks following each other in alternate directions at regular intervals of time. If the galvanometer is provided with a damping arrangement, a final condition will be arrived at in which the galvanometer-needle swings between certain limits. These limits decrease as the interval between the induction-shocks decreases. If, therefore, the rotation of the magnet is rapid enough, the effect of the induced currents on the galvanometer ceases to be visible. It should, however, be remembered that, although the limits between which the galvanometer-needle moves approach zero, the velocity of the needle remains finite. This, of course, is only true if the two induction-shocks are of equal strength. If the induction-current in one direction is stronger than the current in the opposite direction, the galvanometer will show a permanent deflection. As we have two strong currents balancing each other, a very small difference in the resistance will have a strong effect.

II. *Description of Apparatus.*

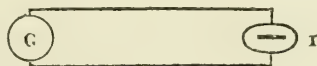
The magnet which was used as electromotive force was fixed to the plate of a siren, which could be set into motion by means of a pair of bellows. The same instrument has been formerly used by R. Kohlrausch and W. Weber*, and later by Kohl-

* "Electrodynamic Measurements, with special reference to the reduction of intensity to absolute measure," *Proc. of the Royal Saxonian Society of Sciences*, vol. iii.

rausch and Nippoldt in a research on the conductivity of sulphuric acid*. I take the following data from the latter paper. The resistance of the wire wound round the magnet is 30 mercury units†. The mean electromotive force of the induction-shocks is

$\frac{n}{342}$ Grove in each direction if the magnet rotates n times in a second. During the following investigation the magnet rotated about forty times a second; so that the resultant electromotive force in each direction was about 0.12 Grove.

The resistance of the galvanometer was found to be about 2477 mercury units; so that the resistance of the whole circuit was as nearly as possible 2500 units. The galvanometer had a plane mirror, and was read off by means of a telescope and scale at a distance. In order to have an idea of the delicacy of the instrument, I measured the deflection produced by a known electromotive force, and I found that the electromotive force of $\frac{1}{1000}$ Daniell caused a first deflection of 200.4 divisions of the scale. The whole arrangement is therefore extremely simple, and is represented by the following diagram:—



G is the galvanometer, I a coil of wires within which the rotating magnet is placed.

III. *Description of Experiments.*

When I first joined the galvanometer to the inductor and rotated the magnet, the effect on the galvanometer-needle was such that I was afraid of a bad contact either in the galvanometer or in the inductor. The needle started wild to one side, then suddenly stopped, turned back to the opposite side, and moved from one side to another without any law. The only regularity I could perceive was that it started always in the same direction. On changing the wires leading to the galvanometer, the needle invariably started to the opposite direction. I broke the connexions and left for about two hours. When I came back every thing had changed. On working the siren the needle now went slowly to one side, and after a few oscillations came to rest at a point about ninety divisions of the scale from the zero-point. On changing the wires leading to the galvanometer the needle

* "On the Validity of Ohm's law for electrolytes, and a numeric determination of the conductivity of sulphuric acid," Pogg. *Ann.* vol. cxxxviii. p. 379 (1869).

† All resistances in this research are referred to mercury units.

went to the other side, and the permanent deflection was numerically the same. The same experiment was repeated several times, and the same deflection was always observed. While thinking over this result, I took the apparatus to pieces, *i. e.* disconnected all wires and joined them again together. The effect had now entirely disappeared, the needle coming to rest exactly at its zero-point. The next day a small unilateral conductivity (as the effect may be properly called) was observed, but after a few experiments disappeared again. During several days I found that this unilateral conductivity generally appeared when the wires had had some rest; and I therefore joined into the circuit different wires which had not been used for some time. Some of these wires showed the effect, and some did not; in all cases it disappeared after several experiments. A wire which had never been used before showed the effect in a remarkable degree. The introduction of this wire, which could not have a resistance larger than 0.1 unit, was sufficient to drive the needle wild to one side. I must mention here a remarkable fact. Suppose we have a circuit in its normal state (that is, showing no unilateral conductivity); let us introduce a wire, and suppose that the unilateral conductivity is now observed. Take the wire out again, so that the circuit is exactly the same as it was before when no unilateral conductivity existed. The unilateral conductivity will now appear, generally even in the same degree as it did with the new wire. If we now by experimenting destroy the unilateral conductivity and join the wire which had caused the disturbance into the circuit again, it will generally behave quite neutral; *i. e.* no unilateral conductivity will be observed. If it do not behave quite neutral, it will only show a small unilateral conductivity, which will be destroyed by a second or third experiment of the same kind.

IV. *Proposed Theory of the Phenomenon.*

It is chiefly the remarkable fact just described (as well as the previous observation, that generally new wires, or such wires as have not been used for some time, showed the effect) that has led me to a theory which, although proved afterwards to be, if not erroneous, at any rate incomplete, explains so well many of the most startling observations that I think it well to give it here. Supposing we pass an electric spark from a sphere to a point, it is known that the distance the electric spark will pass for a given electromotive force is different according as the sphere is positively or negatively electrified. A circuit composed of a metallic wire, terminated at one end by a sphere, separated by a thin layer of air from the other end of the wire would therefore show unilateral conductivity, the positive electricity passing more

easy in one direction through the air than in the other. It is also known that metals condense air in great quantity at their surface; and if we screw two wires with their condensed air together, it is quite conceivable that particles of air will separate the two surfaces of copper, and that a small voltaic arc will therefore be formed. Unilateral conductivity would be the result. If we screw a wire which has air condensed on its surface to a binding-screw, part of the air will pass from the wire to the binding-screw; and it would thus be explained that the temporary addition of a new wire may produce a unilateral conductivity in a circuit which has not shown it before.

V. *Experiments confirming the Theory.*

Many minor coincidences seemed to confirm this theory. Cleaning the ends of the wire with the knife generally destroyed the effect. It was, as a rule, observed in those parts of the circuit which had been disconnected over night. It is always easy to find out in what part of the circuit the effect has its seat. We have only to change the connexions in various places, and to observe in what direction the needle is deflected. I mention one particular case.

The rotation of the magnet one day caused a permanent deflection of the needle of 295 divisions of the scale. On reversing the wires at the ends of the induction-coil, the needle was deflected towards the other side. The effect, therefore, had its seat in the induction-coil. The coil was divided into two halves, which were connected by means of a stout copper wire about half an inch in length. I remembered that this piece of wire had been exposed to the air over night, and I therefore reversed the wire; the needle was deflected 295 divisions of the scale to the other side, showing that my supposition had been correct, and that this small piece of wire, the resistance of which may have been about the hundred-thousandth part of the whole resistance, had caused the deflection. On reversing the wire again, the effect had disappeared.

Another wire was now taken to join the two halves of the induction-coil; a permanent deflection of about 80 divisions of the scale was observed. On cleaning the ends of the wire with a knife the effect disappeared.

These experiments seemed alone sufficient to prove the theory. In order, however, to subject it to a severer test, I thought of condensing air artificially on the surface of the wire. This can readily be done by means of powdered charcoal, which, as is known, absorbs air in great quantity. A wire which was in its normal state was therefore laid with one end into powdered charcoal for about five minutes. When reintroduced into the

circuit, the wire showed a very strong unilateral conductivity. Cleaning and scraping the wire had at first apparently no effect; screwing the wire, however, to another binding-screw attached to the induction-coil destroyed the effect entirely, so that the wire, even when screwed to the original binding-screw, showed no unilateral conductivity. The same experiment was repeated a second time, and with the same result. Five minutes' lying in powdered charcoal was sufficient to reproduce a strong unilateral conductivity; and the same operation as before destroyed it.

VI. *Failure of the Theory.*

A third trial to obtain unilateral conductivity by the same means failed. The wire was put into the charcoal for several hours instead of several minutes; but even then it remained in its neutral state. All the various circumstances which generally had produced unilateral conductivity were now tried; but none succeeded. New wires were tried; the whole apparatus was left untouched and disconnected for several days; but I could not obtain the effect again. I used the same instrument in another investigation during three consecutive weeks, during which various new wires were tried and new combinations employed; but the effect only came out once more, and this time in the galvanometer. The deflection amounted to about 20 divisions of the scale. It lasted for several days and then disappeared.

VII. *Relation of unilateral conductivity to previously known phenomena.*

It is perhaps worth while to say a few words about the relation in which the phenomenon described in these pages stands to other phenomena to which a similar name has sometimes been given. Before attempting to do this, however, it is necessary to allude to one or two objections which might be raised against my interpretation of the experiments described above.

Can the experiments be explained by thermoelectric currents set up by the heating of the wire through the electric vibrations? I think that a careful perusal of the experiments will convince everybody that they cannot be explained that way. I need only draw attention to the unstableness of the effects and to the different facts upon which I thought myself justified in founding the theory mentioned above. These facts certainly cannot be explained by thermoelectric currents.

At first sight my experiments seem to have some relation to a class of phenomena discovered by Poggendorff*, and described

* *Annalen*, vol. xlv. p. 353 (1838), vol. liv. p. 192 (1841).

by him under the name of bilateral deflection (*doppelsinnige Ablenkung*). It seems that the currents in alternate direction affect to a certain degree the temporary magnetization of the needle. This has of course an influence on the time of vibration, which is shorter while the current increasing the magnetism passes through the galvanometer. While the current passes in this direction the needle makes a greater way than in the same time while the current in the opposite direction is passing. The two currents succeeding each other at regular intervals of time will therefore not counterbalance each other, but the current increasing the magnetism of the needle will have the upper hand.

The result will be that the needle will be driven towards the side to which it was originally deflected. This, of course, only happens if the effect of this magnetization is sufficiently strong—that is to say, if the original deflection is sufficiently large; for the magnetizing effect on a needle, placed at right angles to the axis of the galvanometer-coil, is zero, and increases as the sine of the angle of deflection. According to Poggendorff, a needle which is not deflected more than eight or ten degrees from its zero-point, will return to that point if currents in alternate directions are sent through the galvanometer. If, however, the original deflection is greater than 10 degrees, the needle is driven violently towards the side of this deflection.

It is evident that this effect of the electric vibrations is a function merely of the position of the needle; altering the connexions could therefore never produce a reversal of the effect. As, however, I could always drive the needle towards the other side by suitably changing the connexions, this bilateral deflection has evidently had nothing to do with the above experiments.

It remains to say a few words about what has been called unipolar conductivity. This unipolar conductivity has been observed in electrolysis and in flames. The unipolar conductivity in electrolytes has been explained by secondary influences of electrolysis, and, therefore, does not stand in any relation to what I have called unilateral conductivity. The unipolar conductivity of flames has not yet been satisfactorily explained. If my supposition is correct, and if we must look to the air condensed on the surface of the wires for the explanation of unilateral conductivity, it will most likely prove to be closely allied to the unipolar conductivity of flames.

VIII. *Conclusion.*

The result of the foregoing investigation may be perhaps best stated as follows:—

The current produced by an electromotive force in a circuit composed entirely of copper wires joined together by means of binding-screws may, under certain circumstances, be different from the current produced by the same electromotive force acting in the opposite direction.

I have called this phenomenon "unilateral conductivity," and I have tried to bring it into connexion with known facts. The most plausible explanation seemed to me to be, that a thin layer of air may sometimes intervene between the two wires which are screwed together. This explanation has been confirmed by some experiments. Other experiments have shown that the explanation is insufficient. I do not think that the evidence is sufficiently strong to abandon altogether an explanation which seems to agree so well with the most characteristic features of the phenomenon. Secondary causes may intervene which prevent the phenomenon from being formed. I suggest the diffusion of the gases into the wires as such a secondary phenomenon. Effects which are so unstable, however, are never explained by a simple set of experiments. They will only be satisfactorily explained by a number of observations from different experimenters. It is, I hope, a sufficient justification for the publication of the above experiments if they draw the attention of physicists to a class of phenomena which sometimes may seriously interfere with their measurements.

XXXVII. *On the Vibrations of Approximately Simple Systems.*

By LORD RAYLEIGH, M.A., F.R.S.*

IN a paper with the above title, published in the Philosophical Magazine for November 1873, I drew attention to the fact that when the natural vibrations of a system are thoroughly known, the effect of a small variation in the system in changing the types and periods of vibration may be readily calculated by a general method. In particular I proved that the altered periods may be found from the new values of the potential and kinetic energies on the hypothesis that the types are unchanged, subject to an error of the second order only. The present note shows how a further approximation may be made, and how a similar method may be applied to a system subject to small dissipative forces.

If $\phi_1, \phi_2, \&c.$ be the normal coordinates of the original system, the expressions for the kinetic and potential energies are

$$\begin{aligned} T &= \frac{1}{2} [1] \dot{\phi}_1^2 + \frac{1}{2} [2] \dot{\phi}_2^2 + \dots, \\ V &= \frac{1}{2} \{1\} \phi_1^2 + \frac{1}{2} \{2\} \phi_2^2 + \dots \end{aligned} \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (1)$$

* Communicated by the Author.

Now let the system be slightly varied, so that T and V become

$$T + \delta T = \frac{1}{2}([1] + \delta[1])\dot{\phi}_1^2 + \dots + \delta[12]\dot{\phi}_1\dot{\phi}_2 + \dots,$$

$$V + \delta V = \frac{1}{2}(\{1\} + \delta\{1\})\phi_1^2 + \dots + \delta\{12\}\phi_1\phi_2 + \dots,$$

giving for the equations of vibration of the altered system

$$\left. \begin{aligned} ([1]D^2 + \delta[1]D^2 + \{1\} + \delta\{1\})\phi_1 + (\delta[12]D^2 + \delta\{12\})\phi_2 \\ + \dots = 0, \\ (\delta[12]D^2 + \delta\{12\})\phi_1 + ([2]D^2 + \delta[2]D^2 + \{2\} + \delta\{2\})\phi_2 \\ + \dots = 0, \\ \text{\&c.} \end{aligned} \right\} (2)$$

In the original system one of the natural vibrations is that denoted by the sole variation of ϕ_r . In the altered system this will be accompanied by simultaneous *small* variations of the other coordinates. If the whole motion vary as $\cos p_r t$, we get from the *s*th equation, as was proved in the paper referred to,

$$\phi_s : \phi_r = \frac{\delta[rs]p_r^2 - \delta\{rs\}}{[s](p_s^2 - p_r^2)}, \quad . \quad . \quad . \quad (3)$$

an equation which may be regarded as determining approximately the character of the altered types of vibration.

Now the *r*th equation of (2) gives

$$\phi_r(-p_r^2[r] - p_r^2\delta[r] + \{r\} + \delta\{r\}) + \dots + \phi_s(-p_r^2\delta[rs] + \delta\{rs\}) + \dots = 0. \quad (4)$$

Using in (4) the values of $\phi_s : \phi_r$ given in (3), we get for the value of p_r^2 ,

$$p_r^2 = \frac{\{r\} + \delta\{r\}}{[r] + \delta[r]} - \Sigma \frac{(p_r^2\delta[rs] - \delta\{rs\})^2}{[r][s](p_s^2 - p_r^2)}, \quad . \quad . \quad (5)$$

in which the summation extends to all values of *s* other than *r*. The first term in (5) gives the value of p_r^2 calculated without allowance for the change of type, and is sufficient when the square of the alteration in the system may be neglected. If p_r refer to the gravest tone of the system, $p_s^2 - p_r^2$ is always positive, and the term of the second order in (5) is negative, showing that the calculation founded on the unaltered type gives in this case a result which is necessarily too high.

If only the kinetic energy undergo variation,

$$\begin{aligned} p_r^2 &= \frac{\{r\}}{[r] + \delta[r]} - \frac{p_r^4}{[r]} \Sigma \frac{\delta[rs]^2}{[s](p_s^2 - p_r^2)} \\ &= \frac{\{r\}}{[r]} \left\{ \frac{1}{1 + \frac{\delta[r]}{[r]}} - \Sigma \frac{\delta[rs]^2 p_r^2}{[r][s](p_s^2 - p_r^2)} \right\}. \end{aligned} \quad (6)$$

As an example we may take a uniform string of length l and density ρ , carrying a small load m at its middle point. If y be the transverse displacement at point x ,

$$y = \phi_1 \sin \frac{\pi x}{l} + \phi_2 \sin \frac{2\pi x}{l} + \dots, \quad . \quad . \quad . \quad (7)$$

the origin of x being at one end. In this case for the gravest tone we have

$$[r] = [s] = \frac{l}{2} \rho,$$

$$\delta T = \frac{1}{2} m (\dot{\phi}_1 - \dot{\phi}_3 + \dot{\phi}_5 - \dots)^2,$$

so that

$$\delta[r] = m, \quad \delta[rs] = \pm m.$$

Accordingly

$$p_1^2 = P_1^2 \left\{ \frac{1}{1 + \frac{2m}{l\rho}} - \Sigma \frac{4m^2}{l^2 \rho^2 (s^2 - 1)} \right\}, \quad . \quad . \quad (8)$$

since $p_r^2 : p_s^2 - p_r^2 = 1 : s^2 - 1$.

P_r here denotes the value of p_r when there is no load.

Now

$$2\Sigma \frac{1}{s^2 - 1} = \Sigma \frac{1}{s - 1} - \Sigma \frac{1}{s + 1},$$

in which the values of s are 3, 5, 7, 9, &c. Accordingly

$$\Sigma \frac{1}{s^2 - 1} = \frac{1}{4}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

and therefore

$$p_1^2 = P_1^2 \left\{ 1 - \frac{2m}{l\rho} + \frac{3m^2}{l^2 \rho^2} + \text{cubes} \right\}, \quad . \quad . \quad (10)$$

which gives the pitch accurately as far as the square of the ratio $m : l\rho$.

The free vibrations of a system subject to dissipation-forces are determined in general by the equations

$$\frac{d}{dt} \left(\frac{dT}{d\dot{\psi}} \right) + \frac{dF}{d\dot{\psi}} + \frac{dV}{d\psi} = 0, \quad . \quad . \quad . \quad (11)$$

where T and V are as before, and F , called the dissipation-function, is of the form

$$F = \frac{1}{2} (11) \dot{\psi}_2 + \dots + (12) \dot{\psi}_1 \dot{\psi}_2 + \dots * \quad . \quad . \quad (12)$$

* See a paper "On some General Theorems relating to Vibrations," Mathematical Society's Proceedings, June 1873.

of material systems. For the free motion of heat in a conductor, we obtain equations corresponding to those of material systems which are supposed to be devoid of inertia. The functions F and V may thus be reduced to sums of squares; and the effect of a small variation in the system may be investigated by methods parallel to those employed in the present paper.

Terling Place, Witham,
September 11, 1874.

XXXVIII. *On a simple Method of Illustrating the chief Phenomena of Wave-Motion by means of Flexible Cords.* By the late W. S. DAVIS, F.R.A.S., Derby*.

[With a Plate.]

THE simple methods about to be described, of exhibiting the chief phenomena of wave-motion, were suggested during some experiments lately made by the author on the refraction of liquid waves†. These experiments consisted in the production of waves on the surfaces of two liquids of different densities, lying side by side: on agitating the surface of either liquid, waves were produced which passed from one liquid to the other, at the same time undergoing changes in amplitude, length, and form of front. In preparing diagrams to represent these phenomena it became necessary to make drawings of vertical sections through the two liquids, perpendicular to their line of separation.

The appearance presented by the sinuous lines on these diagrams immediately suggested that a similar appearance could be exhibited by means of waves on flexible cords. India-rubber tubes, variously suspended, and both empty and loaded, were tried without satisfactory success; the waves moved too quickly to be well observed, and the reflected waves interfered with the direct ones. Further experiments led the author to devise the simple apparatus now exhibited, which, however, has been made to serve for many other illustrations of wave-motion in addition to those it was at first intended to show.

The apparatus consists essentially of:—(1) a piece of stout board about 20 feet long and 9 inches wide, which should be painted black; and (2) three or four ropes, which must be both heavy and flexible: the ropes used by builders for securing their scaffolding have been found to answer very well, especially if they have been in use some time. To enable the eye to readily

* Read before the Physical Society, May 9, 1874. Communicated by the Society.

† See Brit. Assoc. Report, 1873.

distinguish any particular rope when two or more are used together, it is well to cover the ropes with differently coloured fabrics, say red, blue, and green. A few other accessories are necessary, which will be described as they are required.

By means of this apparatus waves may be produced which move slowly enough to be readily examined by the eye. The chief phenomena of wave-motion which can be shown are as follows :—

1. *Transmission of a Wave.*—One end of a rope, a few feet longer than the board, is fixed to a hook at the end of the board. The free end of the rope is then taken in the hand, and, the rope being quite slack, a sudden up-and-down movement of the hand is made. A protuberance is thus formed which moves very slowly along the rope, presenting the appearance shown in Plate V. fig. 1*.

A single up-and-down movement produces a wave consisting of a crest only, the trough being suppressed by the board; if, however, with the rope very slack, the hand be moved up and down very quickly and energetically, a series of waves, consisting of both crest and trough, are produced (fig. 2).

2. *Amplitude and Wave-length.*—Waves having any length, from 1 to 6 or 7 feet, and amplitudes of similar dimensions, are easily produced by properly controlling the rapidity and energy of the motion of the hand.

3. *Decrease of Intensity with Distance.*—This is illustrated by a succession of waves produced by the well-timed motion of the hand (fig. 2). The actual decrease of amplitude in this case is, of course, due to the loss of energy by friction, and not to lateral spreading.

4. *Relation of Velocity to Elasticity.*—Two similar ropes, one covered with red and the other with blue, are laid side by side along the board and fastened to hooks at one end. The free ends of the ropes are held in the hand, with the finger between them, and, care being taken that they are equally loose, the hand is moved up and down as usual. The result is that a wave of the same height and length is produced on each rope, and the two waves travel side by side to the ends of the ropes. The experiment is repeated with one rope somewhat tighter than the other, when the wave on the tighter rope is observed to travel faster than that on the looser one (fig. 3). On continuing to tighten the rope the velocity of the wave is more and more increased, and may be caused to reach the end of the rope a whole length or more before its fellow.

5. *Relation of Velocity to Density.*—To exhibit this relation a

* The length of the board in the figures is drawn to a much smaller scale than the other parts.

loaded rope is required. That now used has strung upon it a number of rings of lead cut from a leaden water-pipe; these are placed about 6 inches apart, and are covered with india-rubber bands to prevent their making unpleasant noise. The loaded and an unloaded rope are laid on the board side by side, and fixed at one end. Then, taking care the tension is equal in the two ropes, waves are simultaneously generated on them, as before described. It is then observed that the wave on the loaded rope lags considerably behind the other (fig. 4). By sufficiently tightening the loaded rope the velocity of its waves may be made equal to, or even greater than that of the waves of the unloaded rope. This may be used to explain why the velocity of sound in water is greater than in the much less dense medium, air.

6. *Transmission of Waves from one Medium to another of different Density.*—The loaded cord is attached end to end to one much lighter than itself; the united cords are laid on the board with the splice at about the middle of its length. Then, fastening the end of the lighter cord, waves are generated on the heavier one. These waves pass onwards to the lighter cord, on reaching which they acquire greater amplitude, velocity, and length (fig. 5). If the heavier cord be fixed and waves be generated on the lighter one, the reverse changes to those just stated occur on the waves reaching the heavier cord. It is an interesting experiment to transmit waves along a succession of three or more cords alternately heavy and light. With three cords joined end to end, the middle one being heavier than the others, a good illustration is produced of the changes of velocity, length, and amplitude which ætherial waves undergo in passing perpendicularly through a medium with parallel faces.

7. *Separation of a Wave into two or more smaller Waves.*—A single cord extending half the length of the board is joined to a double one extending the other half. Waves are transmitted from the single cord to the double one; on reaching the latter each wave divides in two, one wave traversing one part of the double cord, and the other wave the other part. By giving each part of the double cord a different tension, the velocity of the waves will be different in each (fig. 6). The waves on the double cord may be made to move in planes at right angles to each other by the use of proper guides, thus furnishing an illustration of some of the phenomena of double refraction.

8. *Superposition and Interference.*—The same arrangement is used as in 7, but the waves are transmitted from the double cord to the single one. With equal tension in each part of the double cord, the waves simultaneously produced on each part run side by side until they enter the single cord, when they are su-

perposed and produce a wave of double amplitude. One half of the double cord may be tightened until its wave reaches the single cord half a wave's length before the wave on the other half, when interference occurs, there being little or no lateral motion to be observed in the single cord.

9. *Plane of Waves.*—In the experiments previously described the waves were transmitted in a vertical plane; but by properly directing the motion of the hand, the waves may be transmitted in planes variously inclined to the board, or in a plane parallel with it. Waves in space of three dimensions, corresponding to circularly polarized light, are produced by rapidly and regularly moving the hand in a circle, the cord then taking the form shown at the right of figs. 7 and 9.

10. *Polarization.*—A series of flat boards are used as guides, which are clamped on the long board. These are shown in figs. 7, 8, 9. The vertical and oblique guides are each in two pieces, which are so approximated to each other as to just allow the cord to move freely between them. The horizontal guide is in one piece only. The vertical and horizontal guides being fixed as shown in figs. 7 and 8, waves in a vertical plane are transmitted from that end of the rope nearest the vertical guides; the waves then pass freely through the vertical guides, but are completely stopped by the horizontal one. Waves in a horizontal plane transmitted from the other end of the apparatus pass the horizontal guide, but are stopped by the vertical ones (fig. 8). Waves in an oblique plane transmitted from either end are resolved by the nearest guide into a component in its own plane and a component at right angles which is suppressed; the former passes on and is stopped by the next guide. Circularly polarized waves on reaching the guides are similarly resolved (fig. 7).

11. *Depolarization.*—A pair of oblique guides are required in addition to those described in 10. The arrangement of these is shown in fig. 9, which needs no further explanation. The waves are supposed to proceed from right to left. With a single cord as in fig. 9, or with a partly double one as in fig. 6, an endless variation of experiments relating to polarization may be produced.

12. *Radiation and Absorption.*—A rod of iron about 2 feet in length, having an eye at the centre and at each end, is fixed by means of a screw or pin through the central eye to an upright support of wood clamped at about the middle of the board (fig. 10). The iron rod must be able to rotate freely about the pin in a vertical plane parallel to the board, but in no other plane. Attaching a cord to one end of the iron rod and continuing it to the end of the board, a series of properly timed waves are sent along it, when the rod vibrates in synchronism with the

waves. If a second cord be attached to the other end of the rod and waves be transmitted as before, the vibrations of the rod set up waves in this cord which correspond in period and length to those on the first cord, thus furnishing an illustration of the reciprocity of radiation and absorption.

The author has reason to think that, as nearly all the above-described illustrations have been devised during the last twelve months, the method is capable of much further development and greater perfection.

XXXIX. *Researches in Acoustics*.—No. V.*

By ALFRED M. MAYER†.

1. *An Experimental Confirmation of Fourier's Theorem as applied to the Decomposition of the Vibrations of a Composite Sonorous Wave into its elementary Pendulum-vibrations.*

A SIMPLE sound is a sound which has only one pitch. Such a sound is produced when, with a bow, we gently vibrate the prongs of a tuning-fork and bring them near a cavity which resounds to the fork's fundamental tone. An almost pure simple sound can be obtained by softly blowing a closed organ-pipe. On examining the nature of the vibratory motions of the prongs of the fork‡ and of the molecules of air in the resound-

* This paper is the fifth in the series of those on Acoustics already published in the Philosophical Magazine. The preceding papers, however, were not numbered.

† Communicated by the Author, with corrections, from Silliman's American Journal for August 1874.

Sections 1, 2, 3, 5, 6, and 7 of this paper were read before the National Academy of Sciences during the Session of November 1873. Section 4 was read before the Academy on April 21, 1874.

‡ In my course of lectures on Acoustics, I thus show to my students that the prong of a tuning-fork vibrates like a pendulum:—I take two of Lissajous's reflecting forks, giving, say, the major third interval, and with them I obtain on a screen the curve of this interval in electric light. On a glass plate I have photographed the above curve of the major third passing through a set of rectangular coordinates formed of the sines of two circles whose circumferences are respectively divided into 20 and 25 equal parts. I now place this plate over the condensing-lens of a vertical lantern and obtain on the screen the curve, the circles, and their net of coordinates. Suspended over the lantern is a Blackburn's compound pendulum, which is so constructed that its "bob" cannot rotate around its axis. The bob is hollow, and a curved pipe leads from its bottom to one side of the pendulum. The pendulum is now deflected into a plane at 45° with its two rectangular planes of vibration, so that the end of the curved pipe coincides with the beginning of the curve over the lantern. The bob of the pendulum is fastened with a fine cord in this position, and fine hour-glass sand is poured into it; the cord is now burned, and the sand is delivered from the pipe as the swinging pendulum gives the resultant of its motions in the two planes of vibration, while the photographed curve on the lantern is pro-

ing cavity* and in the closed organ-pipe†, we find that each of these vibrations follows the same law of reciprocating motion as governs the vibrations of a freely swinging pendulum. But other bodies, for instance the free reeds of organ-pipes and of melodeons‡, vibrate like the pendulum; yet we can decompose the vibrations they produce in the air into many separate pendulum-vibrations, each of which produces in the air a simple sound of a definite pitch. Thus we see that a pendulum-vibrating body, when placed in certain relations to the air on which it acts, may give rise to highly composite sounds. It is therefore evident that we cannot always decide as to the simple or composite character of a vibration reaching the ear solely from the determination of the motion of the body originating the sound, but we are obliged to investigate the character of the molecular motions of the air near the ear, or of the motion of a point on the drum of the ear itself, in order to draw conclusions as to the simple or composite character of the sensation which may be produced by any given vibratory motion. Although we cannot often detect in the ascertained form of an ærial vibration all the elementary pendulum-vibrations, and thus predetermine the composite sensation connected with it, yet if we find that the ærial vibration is that of a simple pendulum, we may surely decide that we shall receive from it only the sensation of a simple sound. Thus, if we arm the prong of a tuning-fork with a point, and draw this point on a lamp-blackened surface with a uniform motion and in a direction parallel to the axis of the fork, we shall obtain on the surface a sinusoidal or harmonic curve§; and this curve can only be produced by the prongs of the fork vibrating with the same kind of

gressively covered with the sand if the times of the two vibrations of the pendulum are to each other as 4 to 5.

* Helmholtz, *Tonempfindungen*, 1857, p. 75. Crelle's *Journ. für Math.*, vol. lvii.

† See Mach's *Optisch-akustische Versuche*, Prag, 1873, p. 91. *Die Stroboskopische Darstellung der Luftschwingungen*.

‡ The Rev. S. B. Dod, one of the trustees of the Stevens Institute, has recently made an experiment which neatly shows this:—He silvered the tips of two melodeon-reeds, and then vibrated them in planes at right angles to each other, while a beam of light was reflected from them. The resultant figure of their vibrations is the same as that obtained by two Lissajous's forks placed in the same circumstances and having the same musical interval between them as that existing between the reeds.

§ The equation of this curve is $y = a \sin\left(\frac{2\pi x}{\lambda} + a\right)$. The length, on the axis, of one recurring period of the curve is λ ; the constant a is the maximum ordinate or amplitude. The form of the curve is not affected by a ; but any change in its value slides the whole curve along the axis of x . It is interesting to observe that this curve expresses the annual variation of temperature in the temperate zones.

motion as that of a freely swinging pendulum. If we now bring this vibrating fork near the mouth of a glass vessel whose mass of air responds to the tone of the fork, and, by the method of Mach, examine the vibratory motions of the air, we shall see it swinging backward and forward; and by combining these vibrations with the rectangular vibrations of forks placed outside of the vessel we shall obtain the curves of Lissajous. If the membrane of the drum of the ear be placed in connexion with the resounding cavity, it must necessarily partake of the motion of the air which touches it, and ultimately the auditory nerve fibrillæ are shaken in the same manner, and we receive the sensation* of a simple sound. Here the mind naturally inquires the reason of this connexion existing between the sensation of a simple sound and the pendulum-vibration. It has always appeared to me that the explanation of this invariable connexion is that the pendulum-vibration is the simplest vibratory motion that the molecules of elastic matter can partake of, and that the connexion of the sensation with the mode of vibration is the connexion between the simplest sensation perceived through the intervention of the trembling nerves, and the simplest vibration which they can experience. Indeed the pendulum-vibration is the only one which produces the sensation of sound; for if any other recurring vibration enters the ear, it is decomposed by the ear into its elementary pendulum-vibrations; and if it cannot be so decomposed, then the given vibration is not recurring and does not produce in us the sensation of sound, but causes that which we denominate noise. This remarkable connexion between a simple sound and the pendulum or harmonic vibration, together with the fact of the power of the ear to decompose the motions of a composite sonorous wave into its vibratory elements, was thus distinctly enunciated by Ohm:—*The ear has the sensation of a simple sound only when it receives a pendulum-vibration; and it decomposes any other periodic motion of the air into a series of pendulum-vibrations, each of which corresponds to the sensation of a simple sound.*

We have seen that the harmonic curve is the curve which corresponds to the motion which causes the sensation of a simple sound; but a molecule of vibrating air or a point on the tympanic membrane may be actuated by vibratory motions which, when projected on a surface moving near them, will develop curves which depart greatly from the simplicity of the harmonic, or curve of sines†; but nevertheless these curves

* See Helmholtz on the distinction between a sensation and a perception. *Tonempfindungen*, p. 101.

† In section 6 of this paper I have constructed several important curves corresponding to composite vibrations.

will always be periodic if the sensation corresponding to their generating motions is that of sound. Now Fourier has shown, and states in his theorem, that any periodic curve can always be reproduced by compounding harmonic curves (often infinite in number) having the same axis as the given curve and having the lengths of their recurring periods as $1, \frac{1}{2}, \frac{1}{3}, \frac{1}{4},$ &c. of the given curve; and the only limitation to its irregularity is that its ordinates must be finite, and that the projection on the axis of a point moving in the curve must always progress in the same direction. Fourier demonstrates that the given curve can only be reproduced by one special combination, and shows that, by means of definite integrals, one can assign the definite sinusoids with their amplitudes and differences of phase. Now Helmholtz* has shown that differences of phase in the constituent elementary sounds do not alter the character of the composite sound, and, therefore, that although the forms of the curve corresponding to one and the same composite sound may be infinite in variety (by reason of differences in phase in the component curves), yet the composite sound is always resolved into the same elements. This experimental result of Helmholtz also conforms to the theorem of Fourier in reference to the curves projected by such motions; for he has shown that only one series of sinusoidal resolution is possible.

Fourier's theorem can be expressed as follows:—The constants $C, C_1, C_2,$ &c., and $a_1, a_2,$ &c., can be determined so that a period of the curve can be defined by the following equation†:—

$$y = C + C_1 \sin\left(\frac{2\pi x}{\lambda} + a_1\right) + C_2 \sin\left(2\frac{2\pi x}{\lambda} + a_2\right) + \dots$$

But Fourier's theorem is the statement of a mathematical possibility; and it does not necessarily follow that it can be immediately translated into the language of dynamics without experimental confirmation; for, as Helmholtz remarks, "That mode of decomposition of vibratory forms, such as the theorem of Fourier describes and renders possible, is it only a mathematical fiction, admirable because it renders computation facile, but not corresponding necessarily to any thing in reality? Why consider the pendulum-vibration as the irreducible element of all vibratory motion? We can imagine a whole divided in a multitude of different ways; in a calculation we may find it convenient to replace the number 12 by $8+4$, in order to bring 8

* *Tonempfindungen*, p. 190 *et seq.*

† For other and more convenient forms of expression of this theorem, as well as for a demonstration of it, see pp. 52 and 60 of Donkin's 'Acoustics'—the most admirable work ever written on the mathematical theory of sound.

into view ; but it does not necessarily follow that 12 should always and necessarily be considered as the sum of $8+4$. In other cases it may be more advantageous to consider the number as the sum of $7+5$.

"The mathematical possibility, established by Fourier, of decomposing any sonorous motion into simple vibrations, cannot authorize us to conclude that this is the only admissible mode of decomposition, if we cannot prove that it has a signification essentially real. The fact that the ear effects that decomposition, induces one, nevertheless, to believe that this analysis has a signification, independent of all hypothesis, in the exterior world. This opinion is also confirmed precisely by the fact stated above, that this mode of decomposition is more advantageous than any other in mathematical researches ; for the methods of demonstration which comport with the intimate nature of things are naturally those which lead to theoretic results the most convenient and the most clear."

The theorem of Fourier, translated into the language of dynamics, would read as follows :—" *Every periodic vibratory motion can always, and always in one manner, be regarded as the sum of a certain number of pendulum-vibrations.*"

Now we have seen that any periodic vibratory motion, which has the proper velocity, will cause the sensation of a musical note, and that a pendulum-vibration gives the sensation of a simple sound* ; therefore, if Fourier's theorem is applicable to the composition and decomposition of a composite sonorous wave, it will be thus related to the phenomena of sound :—" *Every vibratory motion in the auditory canal, corresponding to a musical sound, can always, and always in one manner, be considered as the sum of a certain number of pendulum-vibrations, corresponding to the elementary sounds of the given musical note.*"

Heretofore we have called in the aid of the sensations (assumed to be received through the motions of the covibrating parts of the ear) to help us in our determination of the simple or composite character of a given vibratory motion ; but Fourier's theorem does not refer to the subjective effects on the organ of hearing, the dynamic function of whose parts are yet

* Professor Donkin, in his 'Acoustics,' Oxford, 1870, p. 11, advises the use of *tone* to designate a simple sound, and the word *note* to distinguish a composite sound. His reasons are "that *tone* (Gr. *τόνος*) really means *tension*, and the effect of tension is to determine the *pitch* of the sound of a string ;" while a musical *note* is generally a composite sound. Professor Donkin further states, "Helmholtz uses the words *Klang* and *Ton* to signify compound and simple musical sounds. We have followed him in adopting the latter term ; but such a sound as that of the human voice could hardly in English be called a *clang*, without doing too much violence to established usage."

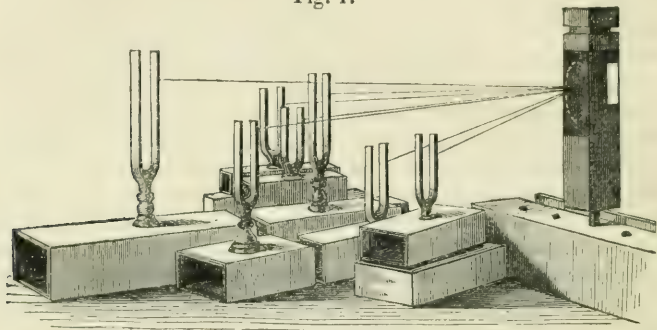
very imperfectly understood. Ohm's theorem, on the other hand, refers entirely to these subjective phenomena of the ear's analysis of a complex sensation into its simple elements. As Fourier's theorem refers only to the decomposition of a composite recurring vibration into its elementary pendulum-vibrations, it has nothing to do with the physiological fact of the correlation of the pendulum-vibrations and the simplest auditory sensation; though this well-ascertained relation gives us the privilege of using this sensation as an indicator of the existence of an aërial pendulum-vibration. Hence, as Fourier's theorem is entirely independent of our sensations, we must endeavour to verify it directly by experiments, which must perform *the actual* decomposition of the composite periodic motion of *a point* into its elementary pendulum-vibrations. But many difficulties present themselves when we would bring to the test of experiment the dynamic signification of Fourier's theorem. For example, the composite sound-vibration, on which we would experiment, emanates from a multitude of vibrating points; parts of the resultant wave-surface differ in their amplitudes of vibration; while points equally removed from one and the same point of the body originating the vibrations, may differ in their phases of vibration; so that when such a wave falls upon covibrating bodies which present any surface, the effects produced are the result of extremely complex motions. The mind sees at once the difference between this complicated conception and the simple one embodied in the statements of the dynamic application of Fourier's theorem.

As the mathematician decomposes *seriatim* every point of the recurring curve into its harmonic elements, so the physicist, in confirming the dynamic application of Fourier's theorem, should decompose into its simple pendulum-vibrations the composite vibratory motion which such a curve represents, and indeed *re-produces* when it is drawn with a uniform motion under a slit in a diaphragm which exposes to view only a point of the curve at once. Therefore only one vibrating point of the composite sonorous wave should be experimented on; and the composite vibratory motion of this point should be conveyed along lines to points of elastic bodies which can only partake of simple pendulum-vibrations. All of these essential conditions I have succeeded in securing in the following arrangement of apparatus.

A loose inelastic membrane (thin morocco leather does well) was mounted in a frame and placed near a reed-pipe; or, as in other experiments, the membrane was placed over an opening in the front of the wooden chamber of a Grenié's free-reed pipe. The ends of several fine fibres from a silk-worm's cocoon were

brought neatly together and cemented to one and the same point of the membrane, while the other ends of these fibres were attached to tuning-forks mounted on their resonant boxes, as shown in fig. 1. In the experiment which I will now describe

Fig. 1.



eight forks were thus connected with one point of the membrane. The fundamental tone of the pipe was Ut_2 , of 128 vibrations per second; and the pipe was brought into accurate unison with a fork giving this sound*. The forks connected with the membrane were the harmonic series of Ut_2 , Ut_3 , Sol_3 , Ut_4 , Mi_4 , Sol_4 , B_4^b , Ut_5 . In the first stage of the experiment we will suppose that the fibres are but slightly stretched; then, on sounding the pipe, all the fibres at once break up into exquisite combinations of ventral segments. If the sunshine fall upon a vibrating fibre and we look on it obliquely in the direction of its length, we shall see ventral segments superimposed on ventral segments in beautiful and changing combinations. On gradually tightening the fibres, we diminish the number of their nodes; and on reaching a certain degree of tension with fibres 1 m. long, I have seen them all vibrating with single ventral segments. On increasing the tension, the amplitudes of these single segments gradually diminish and at last disappear entirely, so far as the unaided eye can discern; and then we have reached the conditions required in our experimental confirmation.

The point of the membrane to which the fibres are attached is actuated by a motion which is the resultant of all of the elementary pendulum-vibrations existing in the composite sonorous wave; and the composite vibrations of this point are

* Since the number of beats per second given by any harmonic (of a pipe out of tune with its harmonic series of forks) will be as the order of the harmonic, it is better to tune a reed to unison with a fork giving one of its higher harmonics. I generally used the Sol_3 fork, or the 3rd harmonic.

sent through each of the fibres to its respective fork. Thus each fibre transmits to its fork the same composite vibratory motion, while each fork can only vibrate so as to give the simple pendulum-vibration of a simple sound; for each fibre is attached to its fork at a point which lies in the upper node of the segments into which the fork divides when it gives its higher harmonic. Now, if Fourier's theorem has "an existence essentially real," any fork will select from the composite vibratory motion which is transmitted to it that motion which it has when it freely vibrates; but if its proper vibration does not exist as a component of the resultant motion of the membrane, it will not be in the least affected. Now this is exactly what happens in our experiment; for when the pipe is in tune with the harmonic series of forks, the latter sing out when the membrane is vibrated; but if the forks be even slightly thrown out of tune with the membrane, either by loading them or by altering the length of the reed, they remain silent when the sounding-pipe agitates the membrane and the connecting fibres*. Thus have I shown that the dynamic application of Fourier's theorem has "an existence essentially real."

It is indeed very interesting and instructive thus to observe in one experiment the analysis and synthesis of a composite sound. On sounding the reed it sets in vibration all the forks of the harmonic series of its fundamental note; and after the reed has ceased to sound, the forks continue to vibrate, and their elementary simple sounds blend into a note which approximately reproduces the timbre of the reed-pipe. If we could by any means obtain all of the elementary vibrations and have them with their relative intensities correctly preserved, we should have an echo of the sound of the reed after the latter had ceased to vibrate; but the impossibility of thus obtaining the highest components of the reed, and the difficulty of reproducing the relative intensities of the harmonics in the covibrating forks, allow us but partially to accomplish this effect.

2. *An Experimental Illustration of Helmholtz's Hypothesis of Audition.*

The experiment which we have just described beautifully illustrates the hypothesis of audition framed by Helmholtz to account for this, among other facts—that the ear can decompose a composite sound into its sonorous elements. Helmholtz founds his hypothesis on the supposition that the rods of Corti, in the ductus cochlearis, are bodies which covibrate to simple sounds—

* See section 5 of this paper for an account of the degree of precision of this method of sonorous analysis.

somewhat, I imagine, as loaded strings* of graded lengths and diameters would act in similar circumstances. The vibrations of the composite wave fall upon the membrane placed near the reed as they fall upon the membrane of the tympanum; and these vibrations are sent through the stretched fibres (or delicate splints of rye-straw, which I have sometimes used) from the membrane to the tuned forks, as they are sent from the membrana tympani through the ossicles and fluids of the ear to the rods of Corti. The composite vibration is decomposed into its vibratory elements by the covibration of those forks whose vibratory periods exist as elements of the composite wave-motion; so the composite sound is decomposed into its sonorous elements by the covibrations of the rods of Corti, which are tuned to the elementary sounds which exist in the composite sonorous vibration. The analogy can be carried yet further by placing the forks in line and in order of ascending pitch, and attaching to each fork a sharply-pointed steel filament. If the arm be now stretched near the forks, so that the points of the filaments nearly touch it at points along its length, then any fork will indicate its covibration by the fact of its pricking the skin of the arm, and the localization of this pricking will tell us which of the series of forks entered into vibration. The rods of Corti shake the nerve-filaments attached to them, and thus specialize the position in the musical scale of the elements of a composite sonorous vibration. Thus a complete analogy is brought into view between our experiment and Helmholtz's comprehensive hypothesis of the mode of audition.

[To be continued.]

XL. *On a Mechanical Principle resulting from Hamilton's Theory of Motion.* By J. J. MÜLLER, Professor at the Polytechnic in Zürich†.

WHEN a system of material points moves under the influence of forces proceeding from the reciprocal attraction and repulsion of the points, all the integral equations of the motion can, as Hamilton has shown‡, be represented by the partial differential quotients of a function of the coordinates (the primary function), in a manner similar to that in which, according to Lagrange, its differential equations can be represented by aid of the partial differential quotients of the force-function. Therein the primary function satisfies two partial differential equations; but even one of these equations, as Jacobi demon-

* For discussions of the vibratory phenomena of loaded strings, see Donkin's 'Acoustics,' p. 139, and Helmholtz's *Tonempfindungen*, p. 267.

† Translated from Poggendorff's *Annalen*, vol. clii. pp. 105-131.

‡ Phil. Trans. 1834, 1835.

strated, is sufficient for its definition. The primary function is a complete solution of this differential equation; and any complete solution of the latter, analogously differentiated according to the constants, gives the system of the integral equations. Hence, in the Hamilton-Jacobi method, the entire problem is concentrated into the one integration of the partial differential equation, in contrast to Lagrange's way of proceeding, in which only single integrals are found by aid of the known principles. The integration of the partial differential equation was developed by Jacobi* generally both in the way already pursued by Lagrange and Pfaff, and also by a new and grand method, both of which methods have been adopted in a series of more recent works.

The theory above mentioned has recently undergone expansion in two respects. If the investigation by Hamilton and Jacobi referred to actual space, for which the element of a line proceeding from a point is capable of being represented by the square root of the sum of the squares of differentials of the ordinates of the point, Lipschitz† formed a more general conception of the problem, inasmuch as he assumed the line-element to be equal to the p th root of any real positive form, of the p th degree, of the differentials of any coordinates of the point in question. The element of its integral corresponding to the primary function becomes the sum of any form of the p th degree of the differential quotients, taken according to time, of the variables and any force-function depending only on the variables—this sum multiplied by the time-element; so that the problem of mechanics is changed into a perfectly general one of the calculus of variations. If, further, Hamilton assumed a force-function which depended only on the coordinates of the moved point, and if Jacobi extended the investigation to a force-function explicitly containing the time, Schering‡ conceived the problem in this direction more generally, introducing forces dependent not only on the position but also on the state of motion of the masses. This dependence is so chosen that, understanding by R the resulting force, and by dr the virtual displacement of the mass-points, $\sum Rdr$ becomes the difference between a total variation and a total derived according to time; and this generalization is at the same time accomplished from Lipschitz's enlarged point of view. In it, therefore, motions can be treated which, for in-

* "Vorlesungen über Dynamik: Nova methodus" &c., Borchardt's *Journal*, 60.

† "Untersuchung eines Problems der Variationsrechnung," Borchardt's *Journal*, 74.

‡ Hamilton-Jacobi'sche Theorie für Kräfte, deren Maass von der Bewegung der Körper abhängt," *Abhandl. der Götting. Ges. der Wissensch.* 1873.

stance, satisfy Weber's law, or motions in Gauss's and Riemann's space of multiple dimensions.

The slight improvement of the physical side of Hamilton's method stands far from the high degree at which these analytical investigations have arrived. An essential peculiarity of it consists in this—that it passes from a given motion of the system of points to another in a similar manner to that in which Lagrange's process passes from one configuration of the points to another. The primary function, a definite integral which is extended over the original motion, undergoes an alteration by the variation of the arbitrary constants of the motion; and this variation, or that of a similar integral representing the expenditure of the force, is given by Hamilton's symbolic equations of motion. Hence Hamilton's method differs, secondly, from Lagrange's (in which the force-function changes according to the elements of the given motion) in the same way as the variation differs from the differentiation of the functions. This second aspect of it could not but lead immediately to a new treatment of the perturbations, which has by Hamilton, Jacobi, and Schering been developed into a series of new systems of perturbation-formulæ. Only the above-mentioned application of the variation of the motion, which is in principle only a particular way of representing the latter, is not the essential of the new view; that must much rather be sought in similar principles to those on which the ordinary differential equations of the mechanical problem are based. It is true that one signification of these principles, the representation of individual integrals, does not here come into consideration in the indicated general process of integration; their physical meaning, however (independent of the other), which was proved most evidently in the proposition of the *vis viva*, especially with the generality given to it by Helmholtz, remains here also; and this justifies an examination of it.

Such an examination of the physical aspect of Hamilton's method is attempted in the sequel. It appeared the more required, as the endeavours of physicists to deduce the second proposition of the mechanical theory of heat in a similar manner as the first, from purely mechanical conceptions, clearly permitted the supposition of a new mechanical principle. Boltzmann*, Clausius†, and Ledieu‡ have succeeded in obtaining from Lagrange's differential equations the proposition mentioned: it did not, however, like the first proposition, come from a universal principle; but, on the contrary, those investigations led to new mechanical propositions, which certainly did not possess

* *Wiener Sitzungsberichte*, vol. liii.; *Pogg. Ann.* vol. cxliii. p. 211.

† *Pogg. Ann.* vol. cxlii. p. 433. *Phil. Mag. S. 4.* vol. xlii. p. 161.

‡ *Comptes Rendus*, 1873, 1874.

the amplitude of the principle of the *vis viva*. An attempt by Szily* to get the proposition out of Hamilton's treatment of the subject comes nearer to the above notion; only, not to mention that, on account of a limitation adhering to the form in which it has hitherto appeared, it could not lead to the general deduction required, it does not approach more closely the physical side of this method.

It resulted from the investigation that the new treatment satisfies a general principle similar to that satisfied by Lagrange's; for perfectly coordinate with the proposition of the *vis viva* is the following:—In a motion whose equations of condition and force-function do not explicitly contain the time, let the primary function and expenditure of force respectively be denoted by V and W , so that

$$-V = \int_0^t (T - U) dt, \quad W = \int_0^t 2T dt,$$

understanding by T the *vis viva*, and by U the force-function; and let it be assumed that V may be represented as a function of the initial and final coordinates and the time, W as a function of the initial and final coordinates and the energy; then for every change of motion occurring during an element of time dt the relation

$$\frac{d(V + W)}{dt} - \left[\frac{\partial (V + W)}{\partial t} \right] = 0$$

holds, in which the symbol d signifies the whole of the alteration which is connected with change of motion, while ∂ denotes all alterations of $V + W$ not produced by variations of the coordinates. Therefore, in every motion whose equations of condition and force-function do not explicitly depend on t , the change of the primary function and force-expenditure produced by the variation of the coordinates alone is $= 0$. The two quantities W and V are here capable of a physical interpretation similar to that of T and U . The former has already been designated by Hamilton as the *vis viva* accumulated in the motion; the signification of the latter results from a peculiarity of the entire Hamiltonian theory of motion: namely, while *la mécanique analytique* prefers to introduce the forces into the equations of motion, Hamilton's treatment involves the introduction of the momentary impulses—indeed, so that the place of the forces is taken by those impulses which at each instant are capable of producing the velocities actually present. Now, in a group of motions, these impulses can, analogously to the forces, be represented as negative partial differential quotients of a function of the coor-

* Pogg. Ann. vol. cxlv. p. 295; vol. cxlix. p. 74. Phil. Mag. S. 4. vol. xliii. p. 339; vol. xlv. p. 426.

dinates; and this function is nothing else but the above-defined primary function of the system. This peculiarity gives to the primary function a real signification similar to that obtained by the force-function in the potential energy, and makes the coordination between the principle of energy and the new proposition still more evident.

If this proposition was the general principle at which those investigations of the theory of heat aimed, it must have included as a special case the second proposition of that doctrine, in the same way as the principle of energy included the first. In this relation it is remarkable that, applied to the mechanical theory of heat, it leads direct to the second main proposition as soon as we make the apparently indispensable supposition that the temperature of bodies is proportional to the *vis viva* of their molecular motion. Corresponding to this, the principle seems also capable of a series of further applications like those of the principle of energy; those which will be here given, however, are limited to the case belonging to the theory of heat.

The proposition cited resulted from the combination of two long-known mechanical equations. Hamilton, namely, had given his equations of motion both in reference to the function V and in reference to the function W ; and the separate results needed only to be combined, in order at once to furnish the new one. It would be obtained in the most general form by introducing the integral elements generalized in the sense of Lipschitz and Schering. As, however, the essential point was its application to real physical motions, and it had to be presented first in its simplest form, I have preferred to give it in connexion with the older method of Hamilton and Jacobi, which moves entirely on this ground. But then this process must in another respect be conceived more generally; for, in every form in which it has hitherto been carried out, it presupposes the force-function unaltered in form with the variation of the motion, while such an alteration of form is sometimes essential in physical considerations. This is the case, for instance, with the molecular motions designated as heat, as soon as the bodies are subjected to changes of volume and pressure. In regard to the quantities accentuated especially by Clausius, which occur together with the coordinates in the force-function, and vary with the variation of the motion, while they remain constant within a given motion, it was therefore needful that the method should be amplified; and this has led to a somewhat more general form of the equations of motion.

§ 1.

Given a system of n material points reciprocally attracting and

repelling, but subject to no other forces, so that the soliciting forces can be represented by the negative partial differential quotients of a function of the coordinates of all the points, the force-function U . This function contains as variable quantities at all events the coordinates q_i of the points in motion, of which it is here always presupposed that they identically satisfy at any moment the equations of condition, of whatever form, and therefore, if m such equations are given, occur to the number of $3n-m=\mu$. Moreover the time t may appear explicitly in the force-function, as well as other quantities c_k , which change only when a transition takes place from one motion to another. For motions of this general sort, Hamilton's method for gaining the general symbolical equation of motion which refers to the variation of the motion is to be extended. If the *vis viva* of the point-system be denoted by T , and the primary function V defined by

$$-V = \int_0^t (T - U) dt,$$

the problem is nearer to that of finding the variation of this integral on the hypotheses made.

In forming this variation, the time t is first regarded as an independent variable which is not varied. All the quantities present in the primary function are therefore regarded as functions of t and a number of arbitrary constants; and from the variation of these constants alone will the variation of those quantities, and hence that of the primary function, result. Of such arbitrary quantities there will always be 2μ in the quantities mentioned, which can be supposed to arise from the integration of the μ differential equations of the second order of the motion; but since a variation of the force-function on the transition from one motion to another is presupposed, to those 2μ constants any number of others may be added; these latter, which at all events are assumed to be independent of one another, are the quantities c_k . If, then, these $2\mu + \nu$ constants change, but t be supposed unchanged, we obtain

$$-\delta V = \delta \int_0^t (T - U) dt = \int_0^t \delta (T - U) dt,$$

and we have only to do with the variation of the quantity $(T - U)$.

Since the equations of condition of the system may explicitly contain the time t , the *vis viva* T will in general, as well as the force-function U , likewise explicitly contain it; but since the time is not varied, in the formation of the total variation δV

which, in consequence of the supposition made, are not connected with one another by any relation. Hence all the variations δq_i , δq_i^0 , δc_k become mutually independent.

In consequence of this, equation (1) can be immediately split up into single equations. Putting, that is to say, the expression which stands under the integral-symbol

$$\Sigma \left[\frac{dp_i}{dt} - \frac{\partial (T-U)}{\partial q_i} \right] \delta q_i = 0,$$

we get the differential equations of the motion

$$\frac{dp_i}{dt} = \frac{\partial (T-U)}{\partial q_i};$$

and as, conversely, the latter are demonstrated by Lagrange to be independent of equation (1), it follows that the expression standing on the right-hand side under the integral-symbol vanishes under all circumstances. Therefore neglecting it, we have

$$-\delta V = \Sigma p_i \delta q_i - \Sigma p_i^0 \delta q_i^0 - \int_0^t \Sigma \frac{\partial U}{\partial c_k} \delta c_k dt; \quad . \quad . \quad (2)$$

and this is Hamilton's symbolic equation expanded. Because, namely, the variations are all independent one of another, they furnish at once the integral equations

$$-\frac{\partial V}{\partial q_i} = p_i, \quad \frac{\partial V}{\partial q_i^0} = p_i^0.$$

Equation (2), with only an unimportant difference in the way of writing it, has already been given by Clausius*; it is, however, to be remarked that his deduction refers only to motions of which the force-functions and equations of condition do not explicitly contain the time. The form in which it gives the variation δV is not sufficiently general for the following considerations, because in general the time t likewise varies, and therewith a partial change is produced both in T and in U and consequently also in V , which is neglected in equation (2).

It shall therefore now be assumed that the time t is no longer the independent variable, but undergoes the change δt on the variation of the motion. In order to understand the sense of this variation, it must be considered that the time is not to be varied wherever it occurs, but only where it occurs explicitly; for a variation of the other would amount to a variation of the initial and final coordinates; and this is already done. In this case, therefore, the primary function V is taken as dependent on the initial and final coordinates, this explicit time t , and the

* Pogg. Ann. vol. cl. p. 122.

quantities c_k ; and their variation is to be formed by varying all these quantities simultaneously. Hence the total variation formed under inclusion of the time becomes

$$-\delta V = \sum p_i \delta q_i - \sum p_i^0 \delta q_i^0 - \int_0^t \sum \frac{\partial U}{\partial c_k} \delta c_k dt - \frac{\partial V}{\partial t} \delta t,$$

and the question is, to determine the last term $\frac{\partial V}{\partial t}$.

In order to obtain this, let it be remembered that, in the differentiation according to t , the quantities c_k contained in the force-function U have been supposed not to vary. From this it follows that

$$\frac{dV}{dt} = \frac{\partial V}{\partial t} + \sum \frac{\partial V}{\partial q_i} \frac{dq_i}{dt};$$

and from this we get immediately the partial differential quotient sought

$$\frac{\partial V}{\partial t} = \frac{dV}{dt} - \sum \frac{\partial V}{\partial q_i} \frac{dq_i}{dt} = U - T + \sum p_i q'_i.$$

If we introduce this value into the above equation for δV , the result is

$$-\delta V = \sum p_i \delta q_i - \sum p_i^0 \delta q_i^0 - \int_0^t \sum \frac{\partial U}{\partial c_k} \delta c_k dt - (U - T + \sum p_i q'_i) \delta t. \quad (3)$$

This general equation relative to the variation of motion, which corresponds to the equations 7** and 7a given by Lipschitz, pp. 122, 123, as well as to Schering's equations [5] and [6], p. 19, containing also the differential equations, is also valid, as soon as a force-function exists, when the force-function and equations of condition explicitly contain the time. For the special case which alone comes into consideration in the following, where the time does not explicitly appear in the force-function and conditions, it takes a somewhat simpler form.

That is, in this case the relation holds,

$$T + U = E,$$

if E denotes the energy of the system. Hence, if we add and subtract, on the right-hand side of the equation for $\frac{\partial V}{\partial t}$, the value $2T$, we get

$$\frac{\partial V}{\partial t} = E + \sum p_i q'_i - 2T.$$

But, with the hypotheses laid down, the *vis viva* becomes a ho-

homogeneous function of the second degree of the variable q'_i ; therefore

$$2T = \sum \frac{\partial T}{\partial q'_i} q'_i = \sum p_i q'_i;$$

consequently we have simply

$$\frac{\partial V}{\partial t} = E,$$

and after substitution in the above equation,

$$-\delta V = \sum p_i \delta q_i - \sum p_i^0 \delta q_i^0 - \int_0^t \sum \frac{\partial U}{\partial c_k} \delta c_k dt - E \delta t. \quad (4)$$

This form, connecting itself with Hamilton's equation*, is the starting-point for the following. At the same time it is significant that the ordinary equations of motion of Lagrange are regarded as satisfied only *in* the motion itself, and not during the change of motion. The system must therefore, *in* the motion, always be a closed one, subject to no action from without; on the contrary, during the variation of motion such an action from without must take place. Meanwhile the energy of the system may remain constant or vary; whether the one or the other, has no influence on the validity of equation (4). This independence of Hamilton's equation upon the nature of the variation of the motion has the same signification as that of Lagrange's equation of motion upon the nature of the variation of the configuration. If, therefore, Lagrange's method reaches to systems with and without conditions, Hamilton's equation (4) extends to systems which with the alteration of their motion retain the energy constant or even receive energy from without.

§ 2.

Hamilton's symbolic equation of motion plays in the treatment of the mechanical problem a part like that of the symbolic equation of motion of Lagrange, only with the difference that it refers to the variation of the motion, while the latter concerns the variation of the configuration in a motion. If, now, in Lagrange's method from the equation of motion a series of principles result which have partly the purely analytical signification of integrals of the differential equations, and partly the essentially physical meaning of general propositions valid for motion generally, the question arises whether similar principles do not connect themselves with Hamilton's equation. This shall be investigated especially in regard to the proposition concerning

* Phil. Trans. 1834, p. 307.

the *vis viva*, which has acquired by far the greatest importance in Lagrange's method.

For that purpose, the already indicated presupposition is made, that in all motions henceforward to be examined time does not occur explicitly, either in the force-function or in the equations of condition; so that Hamilton's equation takes the form

$$-\delta V = \sum p_i \delta q_i - \sum p_i^0 \delta q_i^0 - \int_0^t \sum \frac{\partial U}{\partial c_k} \delta c_k dt - E \delta t. \quad (4)$$

Making use of the well-known substitution given by Euler, and employed also by Hamilton and Jacobi*,

$$V = -W + Et,$$

from which

$$\delta V = -\delta W + E \delta t + t \delta E,$$

this equation of motion changes into

$$\delta W = \sum p_i \delta q_i - \sum p_i^0 \delta q_i^0 - \int_0^t \sum \frac{\partial U}{\partial c_k} \delta c_k dt + t \delta E. \quad (5)$$

Herein

$$\begin{aligned} W = -V + Et &= \int_0^t (T - U) dt + (T + U)t = \int_0^t (T - U) dt \\ &+ \int_0^t (T + U) dt = \int_0^t 2T dt, \end{aligned}$$

and is therefore nothing else but the quantity known under the name of the expenditure of force. It is to be understood as a function of the quantities q_i , q_i^0 , E , c_k ; and the time t , which in the integral in equation (5) remained over, is to be replaced by the equation

$$\frac{\partial W}{\partial E} = t;$$

so that t and E in equations (4) and (5) occupy a perfectly analogous position, in such sort that the one quantity may always replace the other. If now the two relations (4) and (5) be compared, there comes

$$-\left[\delta V - \sum \frac{\partial V}{\partial c_k} \delta c_k - \frac{\partial V}{\partial t} \delta t \right] = \delta W - \sum \frac{\partial W}{\partial c_k} \delta c_k - \frac{\partial W}{\partial E} \delta E. \quad (6)$$

In this equation the variations are still quite undetermined. One of the infinitely many systems of virtual variations will now, under the suppositions made, be the system of the variations which enter with the actual change of motion during the minute portion of time dt . Referred, however, to these actual variations,

* Compare the general transformations of Lipschitz and Schering.

it makes

$$\delta = \frac{d}{dt} dt;$$

and the second and third terms in equation (6) can each, in relation to the coordinates, be conceived as an explicit alteration according to t which may be expressed by

$$\left[\frac{\partial}{\partial t} dt \right].$$

Then comes

$$\frac{d(V+W)}{dt} - \left[\frac{\partial(V+W)}{\partial t} \right] = 0; \quad . \quad . \quad . \quad (7)$$

and this is the proposition sought: *The sum of the alterations in the primary function and the force-expenditure, which are produced by the variation of the initial and final coordinates alone, is, in the variation of every motion that presupposes a force-function and neither contains the time explicitly in this nor in the conditions, equal to nil.*

As the variation of the motion is only subject to the condition that it does not destroy the limitations of the system, but in the rest, as already shown, may very well take place under accession of energy, the proposition we have gained is independent of the special kind of the accession. In this relation the coordination with the proposition of the *vis viva*, which likewise gives the increase of the latter independent of the kind of variation of the configuration, is evident. But this independence forms only one side in the latter proposition; it has received, as is known, another, more important, through the remark that the force-function (in the above representation) is nothing else but the potential energy of the system. In such a new direction the new proposition shall now be investigated.

The sought signification of the primary function readily appears if we give up the forces on which the ordinary theory of motion rests, and introduce in their place momentary impulses capable of producing the velocity existing at any instant. That such a manner of consideration stands in essential connexion with Hamilton's theory of motion has not yet, so far as I know, been rendered evident, although Thomson and Tait have recently* drawn attention to the importance of this second method of procedure, not inferior to the first, and have more nearly completed its theory. In it the components of the momentary impulse (formed according to the general coordinates), if the components of the forces taken according to the rectangular coordinates be

* Treatise on Natural Philosophy, pp. 206 *et seqq.*

introduced into the equation

$$dV = \sum \frac{\partial V}{\partial q_i} dq_i + E dt,$$

we get

$$dV = -2L dt + E dt;$$

and from this,

$$V = \int_0^t (E - 2L) dt, \quad . \quad . \quad . \quad . \quad . \quad (\gamma)$$

a relation which is immediately obtained from the earlier definition-equation of V by making use of the proposition of the energy, was used by Hamilton without hesitation as a definition of the function V , and expresses the proper mechanical meaning of the primary function.

As, then, in (γ) the primary function appears to be formed analogous to the earlier quantity W , it will be convenient to indicate this by a similar notation; and the names of potential and kinetic action may commend themselves for V and W . If we put, moreover,

$$A = V + W = Et,$$

and name this simply the action of the system, equation (7) changes into

$$\frac{dA}{dt} - \left[\frac{\partial A}{\partial t} \right] = 0, \quad . \quad . \quad . \quad . \quad . \quad (8)$$

and the proposition reads:—*That alteration of the action which is conditioned by the variation of the initial and final coordinates alone, vanishes with the change of every motion that presupposes a force-function and does not contain the time explicitly either in this or in the limitations.* In this form it may be designated as the principle of the action.

Here potential and kinetic action are quantities characterizing the given motion in like manner as the potential and kinetic energy the corresponding configuration. If we imagine the whole series of constantly altered motions to be run through, they will in general be distinguished by different values of these quantities: in proportion as, by the mere alteration of the coordinates, the one diminishes, the other increases through the same alteration; so that in this new view perfect correspondence exists with the proposition of the energy.

In regard to the limitations under which the proposition of the action has been obtained, it is to be remarked that the formation of $\int_0^t 2T dt$, like that of T , remains possible even without a force-function. Now the question, what in these cases becomes

of the proposition of the action when there is no force-function, and consequently no primary function, gives occasion to bring out its position to another well-known equation in mechanics, which relates to the above-mentioned momentary impulses.

If, namely, the components of the impulses, formed according to the axes of the rectangular coordinates, are $\Xi H Z$, and the velocity-components induced by them are $x'y'z'$, the equation of motion is

$$\Sigma[(\Xi - mx')\delta x + (H - my')\delta y + (Z - mz')\delta z] = 0.$$

If now as a system of virtual variations the actual alterations of the coordinates be introduced, there results

$$\Sigma(\Xi x' + Hy' + Zz')dt = \Sigma m(x'^2 + y'^2 + z'^2)dt = 2Tdt.$$

Integrated over the given motion, there comes

$$\int_0^t \Sigma(\Xi x' + Hy' + Zz')dt = \int_0^t 2Tdt;$$

and from this results, by variation,

$$\delta \int_0^t \Sigma(\Xi x' + Hy' + Zz')dt = \delta \int_0^t 2Tdt. \quad . \quad . \quad . \quad (9)$$

This is the equation which, in the general case assumed, takes the place of the action-equation; its terms have a similar mechanical meaning to that of the terms of the latter. That is to say, the sum of the left-hand side is nothing else but twice the mechanical work which the sum of the forces constituting an impulse perform during the same. The equation, therefore, immediately passes into

$$\delta \int_0^t 2Ldt = \delta \int_0^t 2Tdt; \quad . \quad . \quad . \quad . \quad (10)$$

and the action-proposition also can be easily brought into this form; for, according to (γ) ,

$$-\delta V + E\delta t + t\delta E = \delta \int_0^t 2Ldt,$$

which, inserted in its equation, furnishes immediately the form (10). The difference between the two cases consists only in this, that in the case of a force-function the terms of the equation are functions of the coordinates, in the other case they are not so—relations analogous to which occur likewise with the proposition of the energy.

§ 3.

In order to illustrate the principle found, which represents a characteristic property of the variations of motion of all systems which satisfy the oft-insisted-on conditions, a simple example,

for which the proposition can readily be verified, may first be discussed. For this I select the motion of a pendulum which takes place in the vertical plane of xy about the downward-directed axis of the positive y in infinitely small amplitudes; and I give the determination of the two functions V and W according to known methods*.

The length of the pendulum being denoted by l , and the elongation each time by θ , so that

$$x = l \sin \theta, \quad y = l \cos \theta,$$

the energy E expressed by the quantities p_i and q_i becomes

$$E = \frac{1}{2} \frac{p^2}{l^2} - gl \cos \theta,$$

where $p = \frac{\partial T}{\partial \theta}$. Accordingly the differential equations of the motion are, taking account of the infinitely small amplitude,

$$\frac{d\theta}{dt} = \frac{p}{l^2},$$

$$\frac{dp}{dt} = -gl\theta;$$

and the two integral equations

$$\theta = \theta_0 \cos \sqrt{\frac{g}{l}} t + \frac{p_0}{l\sqrt{gl}} \sin \sqrt{\frac{g}{l}} t,$$

$$p = p_0 \cos \sqrt{\frac{g}{l}} t - \theta_0 l \sqrt{gl} \sin \sqrt{\frac{g}{l}} t,$$

where θ_0 and p_0 denote the values of θ and p for $t=0$.

Introducing now these values into the expression for the *vis viva*

$$T = \frac{1}{2} \frac{p^2}{l^2},$$

and substituting for the squares and products of the trigonometrical functions the doubled variables, we get

$$T = \frac{p_0^2}{4l^2} + \frac{\theta_0^2 gl}{4} + \left[\frac{p_0^2}{4l^2} - \frac{\theta_0^2 gl}{4} \right] \cos 2 \sqrt{\frac{g}{l}} t$$

$$- \frac{p_0 \theta_0}{2} \sqrt{\frac{g}{l}} \sin 2 \sqrt{\frac{g}{l}} t.$$

* Compare Hamilton's and Jacobi's examples.

When the same values are also introduced into the force-function

$$U = -gl \left(1 - \frac{\theta^2}{2} \right),$$

there results, after a similar reduction,

$$U = -gl + \frac{\theta_0^2 gl}{4} + \frac{p_0^2}{4l^2} + \left[\frac{\theta_0^2 gl}{4} - \frac{p_0^2}{4l^2} \right] \cos 2\sqrt{\frac{g}{l}} t \\ + \frac{p_0 \theta_0}{2} \sqrt{\frac{g}{l}} \sin 2\sqrt{\frac{g}{l}} t;$$

and inserting, finally, both these values in the primary function

$$-V = \int_0^t (T - U) dt,$$

we obtain

$$-V = glt + \left[\frac{p_0^2}{4l\sqrt{gl}} - \frac{\theta_0^2 l \sqrt{gl}}{4} \right] \sin 2\sqrt{\frac{g}{l}} t \\ + \frac{p_0 \theta_0}{2} \left(\cos 2\sqrt{\frac{g}{l}} t - 1 \right),$$

or, if by aid of the first of the integral equations we put

$$p_0 = \frac{l\sqrt{gl}(\theta - \theta_0) \cos \sqrt{\frac{g}{l}} t}{\sin \sqrt{\frac{g}{l}} t},$$

and, lastly, introduce again into the trigonometrical functions the simple variables,

$$-V = glt + \frac{1}{2}(\theta^2 + \theta_0^2) l\sqrt{gl} \cot \sqrt{\frac{g}{l}} t - \frac{\theta \theta_0 l \sqrt{gl}}{\sin \sqrt{\frac{g}{l}} t}. \quad (11)$$

Referred to the same variables l and θ , we have on the other hand

$$\frac{\partial T}{\partial \theta'} = l^2 \theta', \\ \theta' = \frac{1}{l^2} \frac{dW}{d\theta}, \\ T = \frac{1}{2l^2} \left(\frac{dW}{d\theta} \right)^2.$$

If this, together with the value of U , be inserted in the known differential equation for Λ ,

$$T = -U + E,$$

the result is

$$\frac{1}{2l^2} \left(\frac{dW}{d\theta} \right)^2 = gl \left(1 - \frac{\theta^2}{2} \right) + E;$$

and from this the integral

$$W = \int_{\theta_0}^{\theta} \sqrt{2gl^3 + 2l^2E - gl^3\theta^2} d\theta. \quad (12)$$

If we now form out of (11) and (12) the differential quotients

$$\begin{aligned} -\frac{\partial V}{\partial \theta} &= \frac{\theta l \sqrt{gl}}{\tan \sqrt{\frac{g}{l}} t} - \frac{\theta_0 l \sqrt{gl}}{\sin \sqrt{\frac{g}{l}} t}, & -\frac{\partial V}{\partial \theta_0} &= \frac{\theta_0 l \sqrt{gl}}{\tan \sqrt{\frac{g}{l}} t} - \frac{\theta l \sqrt{gl}}{\sin \sqrt{\frac{g}{l}} t}, \\ \frac{dW}{d\theta} &= \sqrt{2gl^3 + 2l^2E - gl^3\theta^2}, & \frac{dW}{d\theta_0} &= -\sqrt{2gl^3 + 2l^2E - gl^3\theta_0^2}, \end{aligned}$$

and introduce them into the action-equation, we get

$$\begin{aligned} l \sqrt{gl} \left(\theta \frac{d\theta}{dt} + \theta_0 \frac{d\theta_0}{dt} \right) \frac{\cos \sqrt{\frac{g}{l}} t - 1}{\sin \sqrt{\frac{g}{l}} t} &= \sqrt{2gl^3 + 2l^2E - gl^3\theta^2} \frac{d\theta}{dt} \\ &\quad - \sqrt{2gl^3 + 2l^2E - gl^3\theta_0^2} \frac{d\theta_0}{dt}. \quad (13) \end{aligned}$$

Indeed it can be readily shown that the individual derivata are p and p_0 respectively. For if the quantity p_0 be eliminated from the second of the integral equations for example by aid of the first, we get immediately

$$p = \frac{\theta l \sqrt{gl}}{\tan \sqrt{\frac{g}{l}} t} - \frac{\theta_0 l \sqrt{gl}}{\sin \sqrt{\frac{g}{l}} t} = -\frac{\partial V}{\partial \theta};$$

and if we put this value of p in the expression of the energy, it changes into

$$E = \frac{gl^3\theta^2 \cos^2 \sqrt{\frac{g}{l}} t - 2gl^3\theta\theta_0 \cos \sqrt{\frac{g}{l}} t + gl^3\theta_0^2}{2l^2 \sin^2 \sqrt{\frac{g}{l}} t} - gl \left(1 - \frac{\theta^2}{2} \right);$$

and hence

$$\frac{\partial W}{\partial \theta} = \sqrt{\frac{gl^3\theta^2 \cos^2 \sqrt{\frac{g}{l}} t - 2gl^3\theta\theta_0 \cos \sqrt{\frac{g}{l}} t + gl^3\theta_0^2}{\sin^2 \sqrt{\frac{g}{l}} t}} = p.$$

Of the applications of the proposition of the action, those shall be introduced here which can be made of it in the mechanical theory of heat. If heat be conceived as molecular motion, the application to it of the Energy proposition leads immediately to the first main proposition of this doctrine. Corresponding to this, we are now investigating what, on the same hypothesis, results from the Action theorem. These molecular actions are stationary motions of a system of points; and the simplest case of such motions is obviously that in which all the points move in closed paths, and with a period common to all of them. This shall first be supposed.

As, for closed paths, the two limits of the integral which forms the action coincide, when the integration is extended over an entire revolution we obtain

$$\sum \frac{\partial W}{\partial q_i} \frac{dq_i}{dt} - \sum \frac{\partial W}{\partial q_i^0} \frac{dq_i^0}{dt} = \frac{dW}{dt} - \left[\frac{\partial W}{\partial t} \right] = 0;$$

and hence the equation of the Action proposition is transformed into

$$-\frac{dV}{dt} + \left[\frac{\partial V}{\partial t} \right] = 0,$$

or, written explicitly,

$$-dV + \sum \frac{\partial V}{\partial c_k} dc_k + E dt = 0.$$

If now, for one revolution, we name the mean value of the *vis viva* \bar{T} , and that of the force-function \bar{U} , we obtain

$$\begin{aligned} -V &= \int_0^t (t - U) dt = t(\bar{T} - \bar{U}), \\ -dV &= t d\bar{T} - t d\bar{U} + \bar{T} dt - \bar{U} dt, \\ \sum \frac{\partial V}{\partial c_k} dc_k &= \int_0^t \left[\sum \frac{\partial U}{\partial c_k} dc_k \right] dt = t \sum \frac{\partial \bar{U}}{\partial c_k} dc_k, \\ E &= \bar{T} + \bar{U}; \end{aligned}$$

and if we insert this value in the above equation, we obtain

$$t d\bar{T} - t d\bar{U} + 2\bar{T} dt + t \sum \frac{\partial \bar{U}}{\partial c_k} dc_k = 0,$$

from which

$$d\bar{U} - \sum \frac{\partial \bar{U}}{\partial c_k} dc_k = d\bar{T} + 2\bar{T} d \log t, \quad . \quad . \quad (14)$$

a well-known equation, already advanced by Clausius* for such motions.

If now we apply this or related equations to the molecular

* Pogg. *Ann.* vol. cxlii. p. 433. *Phil. Mag.* S. 4. vol. xlii. p. 161.

motion designated heat, making use at the same time of the hypothesis that the temperature is proportional to the *vis viva* of the motion, we arrive (as Boltzmann, Clausius, and Ledieu have shown) easily at the second proposition of the mechanical theory of heat. In general, however, the motion of the molecules of a body does not take place in closed paths. With respect to fluids, for example, we are not even justified in assuming for them a fixed mean position; and in the case of solids, where such an assumption is indeed necessary, the actual motion will yet be distributed along all the dimensions. Now, for such cases Clausius has recently called attention to a second, analogous equation, which substitutes another hypothesis for that of closed paths. A more direct derivation of the second main proposition from the theorem of the action shall here be given.

The suppositions which have been made respecting the system of points representing the body are simply that the motion is a stationary one, and that it is infinitesimally changed by the communication of an elementary quantity of heat. The subject of investigation is the quantity

$$\Sigma p_i \frac{dq_i}{dt} - \Sigma p_i^0 \frac{dq_i^0}{dt},$$

which refers to the variation mentioned. Since infinitesimal alterations of the velocities in the time-particle dt produce only infinitesimal path-changes of the second order, this makes

$$\Sigma p_i^0 \frac{dq_i^0}{dt} = \Sigma p_i^0 q_i'^0.$$

Further, the system of variations $\frac{dq_i}{dt}$ can be split into two.

Let the first be the distances $q_i dt$ which are traversed in the original motion during the time-element dt from the points q_i . This portion furnishes the sum

$$\Sigma p_i q_i' = \Sigma p_i^0 q_i'^0.$$

Let the second partial system be the distances $\epsilon_i dt$ which lead from the above-mentioned last positions in the original motion to the final positions in the changed motion. In it, under the suppositions made, to every value of p there come just as many positive as negative ϵ ; this portion therefore furnishes the sum

$$\Sigma p_i \epsilon_i = 0.$$

Accordingly, for the infinitesimal variation which in the stationary motion of the point-system is conditioned by an infinitely small quantity of heat,

$$\Sigma p_i \frac{dq_i}{dt} - \Sigma p_i^0 \frac{dq_i^0}{dt} = 0.$$

Therefore, introducing the function V ,

$$\frac{dV}{dt} - \left[\frac{\partial V}{\partial t} \right] = 0;$$

and the equation of the action can be written

$$\frac{dW}{dt} - \sum \frac{\partial W}{\partial c_k} dc_k - t \delta E = 0.$$

But since

$$W = \int_0^t 2T dt = 2t\bar{T},$$

$$dW = 2t d\bar{T} + 2\bar{T} dt,$$

$$\sum \frac{\partial W}{\partial c_k} dc_k = -t \sum \frac{\partial \bar{U}}{\partial c_k} dc_k,$$

we have

$$2t d\bar{T} + 2\bar{T} dt + t \sum \frac{\partial \bar{U}}{\partial c_k} dc_k - t \delta E = 0;$$

and from this

$$dE - \sum \frac{\partial \bar{U}}{\partial c_k} dc_k = 2d\bar{T} + 2\bar{T} d \log t,$$

or

$$dE - \sum \frac{\partial \bar{U}}{\partial c_k} dc_k = 2\bar{T} d \log (t\bar{T}). \quad . \quad . \quad . \quad . \quad (15)$$

Now this equation, which has already been given by Szily* for the special case in which no c_k are present, the paths are closed, and the periods are the same for all the points, leads immediately to the second proposition of the mechanical theory of heat. For that purpose let us consider, first, that the left-hand side of it is nothing else but the energy which, with the change of the molecular motion, is communicated to the body as heat from without; and therefore, in the usual notation of the theory of heat, it is $\frac{g}{\Lambda} dQ$. If we then make use of the assumption that \bar{T} is proportional to the absolute temperature Θ , we immediately obtain

$$\frac{dQ}{\Theta} = dS, \quad . \quad . \quad . \quad . \quad . \quad (16)$$

understanding by dS a complete differential.

Thus the Second Proposition is derived, like the First, from a general mechanical principle. But the above representation permits us to perceive for the two propositions not merely this

* Pogg. *Ann.* vol. cxlv. p. 295. Phil. Mag. S. 4. vol. xliii. p. 339.

conformity of position, but also a common origin. The development of the variation of Hamilton's integral has the peculiarity that it leads simultaneously to the differential and integral equations of mechanical problems. This remarkable fact gives to the principles of Energy and Action a common origin in the general equation of motion; and by this the latter becomes the connecting band for the two propositions of the mechanical theory of heat.

Zürich, April 1874.

XLI. On a New Formula in Definite Integrals.

To the Editors of the *Philosophical Magazine and Journal*.

11 Elysium Row, Calcutta,
August 2, 1874.

GENTLEMEN,

I SEE in the July Number of your Magazine two new formulæ in definite integrals of some importance are given by Mr. Glaisher. The integrals admit of a direct general solution without using the identity on the right-hand side of the equation,

$$a_0 - a_1 x^2 + a_2 x^4 \&c. = \frac{a_0}{1+x^2} - \frac{\Delta a_0 x^2}{(1+x^2)^2} \&c.$$

The portion to the left is evidently equal to

$$\frac{1}{1 + \epsilon^{D_a} x^2} \cdot a_0;$$

or putting $E = \epsilon^{D_a}$,

$$\frac{1}{1 + E \cdot x^2} a_0 = u \text{ say.}$$

From this

$$\int u dn = \int \frac{dn}{1 + E x^2} \cdot a_0 = \{E^{-\frac{1}{2}} \cdot \tan^{-1} E^{\frac{1}{2}} x\} a_0.$$

And taking the limits α and 0, the value for this particular case is

$$E^{-\frac{1}{2}} \cdot \frac{\pi}{2} \cdot a_0 = \frac{\pi}{2} \cdot a_{-\frac{1}{2}}.$$

The second theorem is obtainable in the same way. It is $\int \frac{u dx}{1+x^2}$, or

$$\begin{aligned} & \int \frac{dn}{1+x^2 \cdot (1+E x^2)} \cdot a_0 \\ &= (1-E)^{-1} \cdot \left\{ \int \frac{dn}{1+x^2} - E \int \frac{dn}{1+E x^2} \right\} a_0 \\ &= (1-E)^{-1} \cdot \{ \tan^{-1} x - E^{\frac{1}{2}} \tan^{-1} E^{\frac{1}{2}} x \} a_0. \end{aligned}$$

This is the general solution ; and where the limits are infinity and cipher, there results

$$(1-E)^{-1} \cdot \frac{\pi}{2} \cdot \{1 - \epsilon^{-\frac{1}{2}}\} a_0 = \frac{\pi}{2} \cdot \{1 + E^{\frac{1}{2}}\}^{-1} a_0,$$

which is the same as given by Mr. Glaisher.

It is evident that there are numerous theorems of the same kind, such as

$$\cos Ex \cdot a_0 = a_0 - \frac{a_2 x^2}{1 \cdot 2} + \&c.,$$

$$\sin Ex \cdot a_0 = a_1 x - \frac{a_3 x^3}{1 \cdot 2 \cdot 3} + \&c.,$$

which will give definite results between the limits infinity and cipher.

Yours obediently,

JAMES O'KINEALY, B.C.S.

XLII. On an Absolute Galvanometer.

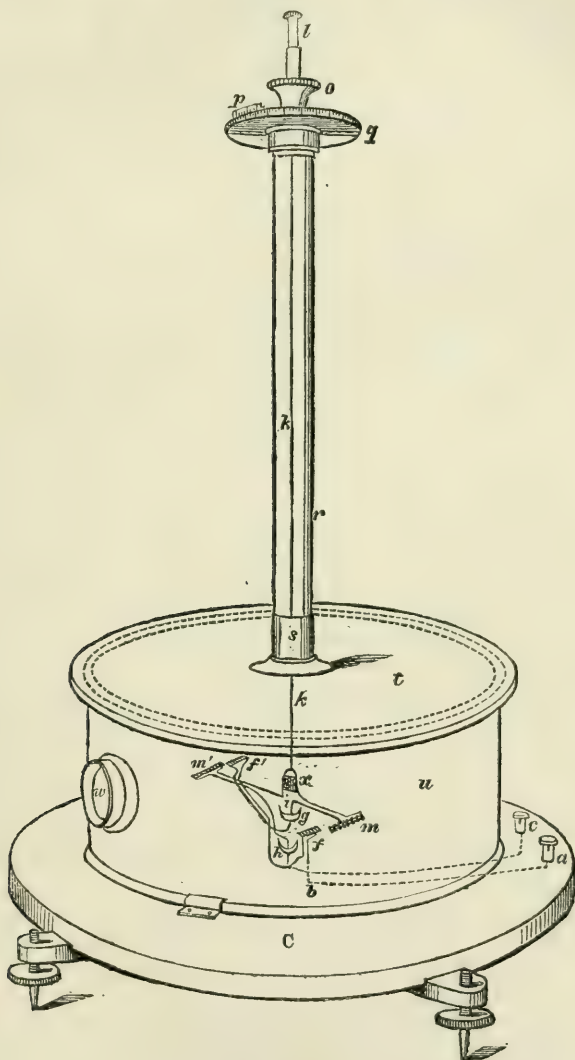
By FREDERICK GUTHRIE*.

MESSRS. ELLIOTT have constructed for me a galvanometer which will, I believe, be found to possess for some purposes certain advantages over those at present in use. Its principle depends upon the measurement of the current-strength by the measurement of the mechanical force necessary to bring to a given distance of one another two electromagnets, which are excited by the current in such a fashion that they repel one another.

The current enters at *a* by the screw-clamp ; thence it passes beneath the circular wooden stand *C* along the copper wire *ab*. It rises vertically and coils round a soft iron mass *f*, which lies horizontal and tangential to the axis of the instrument. It passes down and across the centre of the board, then rises and coils round a soft iron mass *f'*, exactly similar and similarly placed to *f*, but on the opposite side of the instrument. Having encircled *f'*, the current-bearing wire again descends, and carries a mercury-cup *g*, through whose bottom it passes, and which is exactly in the axis of the instrument. The current then leaves the mercury by the wire *i*, which dips into it. It then traverses the wire around the iron, *m*. Thence it crosses the instrument and forms a spiral around *m'*, after which it passes into the mercury-cup *h*, and so to the binding-screw *c*. The spirals are such

* Read before the Physical Society, May 23, 1874. Communicated by the Society.

that there is repulsion between f and m , and also between m' and f' . It is seen that the magnetic pair ff' is fixed. The pair



$m m'$ is movable about a vertical axis. The system $m n'$ is hung by a metal or glass thread k from the rod l , which works stiffly through the nut o . The latter carries an arm and vernier, p , which slides over the graduated head, q . The scale, nut, &c.

are supported on the glass tube r , which is fastened by the cap s on to the plate-glass disk t , which rests upon the top of the glass cylinder u clamped upon the wooden base c resting on levelling-screws. In the side of u is a plate-glass window, w , through which a vertical line of light may be focused upon x (a mirror fastened to the mm' system), and thence thrown upon a scale in the manner which is now so often employed.

A word or two about the way in which the instrument is used. The upper plate t and the system mm' are removed by lifting r . The edge of u is rubbed with beeswax to prevent t from slipping upon it. The copper wires penetrating the cups are amalgamated and a little mercury poured in. Amalgamated thin platinum-foil is then pressed into the cups, and mercury is poured upon this. By this means a concave meniscus is obtained. The upper part is then replaced, and so adjusted by turning the plate t and the cylinder u that the mirror x is parallel to the window w , when the axis of mm' makes an angle of about 15° with that of ff' . The rod l is adjusted so that the wires of mm' just touch the mercury; and by the levelling-screws k is so swung that m and f , and also m' and f' , are exactly opposite to one another and the wires in the centres of the mercury-cups. A slit of light is then sent through w , reflected on to a screen, and the head o is then turned till the slit is split by an arbitrary vertical line on the screen. The reading of p is then noted. A current passing through the system forces mm' away from ff' . Turn the head o until the slit of light is again brought to the mark on the screen. The angle through which it must be turned is directly proportional to the magneto-repulsion—that is, to the square of the current-strength. Many of the laws of electrodynamics may be readily illustrated by this instrument; and not only may different currents be compared with the greatest accuracy, but the absolute mechanical magneto-value of the current may be at once arrived at. By bringing the repellent magnets always to the same distance from one another, a whole class of sources of error is removed.

XLIII. Notices respecting New Books.

First Lessons in Theoretical Mechanics. By the Rev. JOHN F. TWISDEN, M.A., Professor of Mathematics in the Staff College, and formerly Scholar of Trinity College, Cambridge. London: Longmans, Green and Co. 1874: pp. 243.

THOSE teachers and students who are already acquainted with the author's large *Treatise on Mechanics*, will naturally expect to find in the work now before us perfect exactitude both in

reasoning and expression; nor will they, making due allowance for the difficulty of the undertaking, be in any way disappointed. It is not an easy task to teach even the first principles of mechanics to those of whom only a knowledge of Arithmetic, a little Geometry, a few rules of Mensuration, an aptitude in the use of compasses, scale, and protractor, and enough Algebra to solve a simple equation are demanded. Yet the author has performed this task in a manner which shows that with him teaching is an art of which he is an accomplished master. It is true that now and then he is obliged to omit or postpone the proofs of certain important theorems which involve a knowledge of Geometry and Trigonometry not possessed by beginners. In the parallelogram of forces (art. 37), for instance, the student is told to find the resultant by construction. That the resultant is the diagonal of a parallelogram of which the two given forces are adjacent sides, is assumed to be true—the reason of the rule being given in a subsequent chapter (137), to which, however, no clue is given. And this seems a suitable place, in our notice of Mr. Twisden's book, for remarking that a work containing so much matter (far more than at first sight appears) ought certainly to be furnished with a copious index.

That the centre of gravity of a triangle is the intersection of the three straight lines which join the vertices to the middle points of the opposite sides, is a proposition also given without proof (art. 18), showing that the little Geometry which Mr. Twisden requires of his readers does not even extend to the proof of so simple a theorem. In art. 18 (*b*) we are told that "any area may be conceived to be made up of a number of parallel straight lines," a conception which must be inconsistent with the young geometer's notion of a straight line. By use of the principle of limits in finding the centre of gravity of the surface of a triangle, this inconsistency would certainly be avoided.

The book consists of eight chapters, the first five of which are made as simple as possible. Each chapter is followed by a collection of excellent questions, not less than four hundred altogether being given in this manner. Besides, nearly two hundred complete solutions of useful and interesting problems are scattered throughout the book, invaluable to those who study without a teacher.

There are also Tables of Specific Gravities, Moduli of Elasticity, Tenacities, and Resistances to Compressions.

We recommend the book to the notice of that numerous class for whom it is specially intended—those who must know mechanics and yet possess but little mathematical knowledge—to Students, as being suitable to the curriculum of the University of London, and to all Teachers, on account of the always clear, and often ingenious, developments of the most important parts of the subject.

Supplement to the First Book of Euclid's Elements, containing the Sixth-Book Propositions proved independently of the Fifth Book, and the Elementary Propositions of Modern Geometry. By EDWARD BUTLER, M.A.T.C.D. Dublin: Alexander Thom. 1872 (12mo, pp. 60).

Euclidian Geometry. By FRANCIS CUTHBERTSON, M.A., late Fellow of Corpus Christi College, Cambridge, Head Mathematical Master of the City of London School. London: Macmillan and Co. 1874 (fcap. 8vo, pp. 266).

The titlepages of these works sufficiently indicate their purpose; they are intended to be substitutes for Euclid's Geometry, or for part of it, and while retaining the form and spirit of the original, to improve on it in detail and to supplement its supposed deficiencies. Both books are, to all appearance, written by mathematicians of average competency; and one, at least, of the authors (Mr. Cuthbertson) is in a position which makes it highly probable that he is a teacher of considerable experience*. But though this is the case, we regret to have to add that, after having looked into these books with some care, we do not see why they have been published.

With Mr. Butler's book the difficulty is not so great as with Mr. Cuthbertson's. We may surmise that it is adapted to some course of instruction sanctioned by the Commissioners of National Education in Ireland; and if so, its somewhat fragmentary form is explained. It consists of a number of propositions designed as a substitute for Euclid's sixth book, and a selection of elementary propositions on Harmonic and Anharmonic Section and some allied subjects. It is hardly necessary to notice this book further; and were we asked for the reason, we should regard it as a sufficient answer to state that Mr. Butler's definition of proportion runs thus:—"Four straight lines are said to be *proportionals*, that is, the same ratio the first to the second as the third to the fourth, when the rectangle contained by the first and fourth is equal to the rectangle contained by the second and third." Putting out of the question the typographical error which may be presumed to exist in the passage, the sentence betrays a view of the function of definition which is above or below criticism.

Mr. Cuthbertson's book covers just the same ground as Euclid's Books I-6, and the first twenty-one propositions of Book II. He has manifestly expended a great deal of care and thought on its composition, and yet we are constrained to say that his attempt to improve on Euclid is a failure. In the first place, his book is about as long and quite as abstruse as Euclid's. In the next, he has increased the difficulty of his task by adapting his book to a certain form of examination which our limits will not allow us to

* We do not know what position Mr. Butler holds; he calls himself "Professor &c. under the Commissioners of National Education in Ireland." We are wholly in the dark as to the meaning of the "&c."

explain. Then, again, the substance of his book seems to us of a far inferior quality to Euclid's: this is no more than might be expected; but we will give an instance of what we mean. Euclid's treatment of the Corollaries to the 32nd prop. of Book I is not, perhaps, wholly proof against minute criticism; still if any thing be wanting it could be supplied by a word or two of explanation; and surely nothing can be plainer or more direct than his method. Mr. Cuthbertson, however, wishes to improve upon it, and he does so as follows:—On p. 39 he gives a Corollary, which is stated thus:—"If AB, BC are two straight lines respectively parallel to DE, EF , then shall the angle ABC be equal to the angle DEF ." This is true or not according to the direction in which EF is drawn: *e. g.* it is true in the case shown in Mr. Cuthbertson's diagram; but the needful qualification is not given in the Corollary, nor, so far as we have noticed, anywhere else. On p. 48 this Corollary is used to prove the theorem "if the sides of a polygon be produced in order, the exterior angles shall together be equal to four right angles." The proof consists in taking a point outside the polygon and drawing from it rays parallel to sides respectively. This proof, of course, may be made perfectly sound; but in the case before us it fails owing to the above-mentioned ambiguity. This is the way in which he treats Euclid's second Corollary, and then he goes on to prove Euclid's first Corollary. The method is in no respect better than Euclid, and the way of stating it inferior to the extent of inaccuracy.

There is one question of general interest, suggested by a perusal of Mr. Cuthbertson's book, on which we will say a few words, viz. "What are axioms?" To the mathematician they are merely truths of geometry assumed without proof, as premises needful for proving other truths of geometry. It is usual to answer that axioms are self-evident truths. But, not to say that the question at once arises "Self-evident to whom?" it is to be observed that the question "How do we come by our knowledge of the axioms of geometry?" is one with which the mathematician, as such, has nothing to do. There are, of course, two distinct ways of answering this question, and each doubtless capable of numerous modifications. Some hold that the axioms of geometry are what they are in virtue of the conformation of the mind antecedently to all experience of space. Others hold that the axioms are nothing but the expression of our most elementary experiences of space, and that what is called their necessary truth is merely a consequence of the uniformity of our experiences, joined to the absence of any experience which suggests so much as a type of something inconsistent with them. We believe this to be a sufficiently correct, though brief, statement of the two rival answers; and the observation we have to make on them is, that whether either or neither of them be true is a question wholly outside of geometry.

We may not, perhaps, be justified in doing more than suspecting (but at all events we do very strongly suspect) that the reason of Euclid's 12th axiom being so much objected to is that many mathe-

maticians regard the former as the correct answer to the above question. There is not much difficulty in believing that we are born into the world with minds so constituted that as soon as we know the meaning of words we cannot do otherwise than hold that things equal to the same thing are equal to one another; but no one except a hardened metaphysician could suppose that a belief of the 12th axiom is produced by any thing but an acquaintance with the actual properties of space. Accordingly many wish to substitute for it something which is more "self-evident," *i. e.* something more consonant with their metaphysical views.

It is not easy to see, on other grounds, what advantage is gained by substituting one axiom for another. No one has any difficulty in understanding what the 12th axiom means, nor in seeing that it is undoubtedly true. If any one will prove the converse of proposition 27 without assuming more than the first eleven axioms and the first 27 propositions, he will do something worthy of all honour. But when the question is to prove the point by means of a special axiom which differs from Euclid's our interest in the matter is but small. *e. g.* If any one prefers Playfair's axiom to Euclid's we do not know why he should not; only we would remark that it is merely a question of preference, that the two axioms are quite coordinate with each other, and that if either is taken for granted the other can be immediately proved.

Mr. Cuthbertson, however, takes a different view from this, and he goes to work to improve upon Euclid as follows:—On p. 33 he gives "Deduction G," viz. "If points be taken along one of the arms of an angle farther and farther from the vertex, their distances [meaning, as explained, perpendicular distances] from the other arm will at length be greater than any given straight line." It is obvious that this statement as it stands is not true; however, the needful correction could be supplied without much difficulty; *e. g.* it would be sufficient for present purposes for it to run, "If points be taken at equal distances &c.," and this is apparently what is meant. Further, the demonstration of the deduction assumes that any angle however small can be multiplied until an angle is obtained greater than a right angle. We have no objection to this being assumed, only to its being assumed implicitly. In a book which formally specifies the axioms assumed, it ought to have been separately enunciated as an axiom; and we cannot find that this has been done. On p. 34 Mr. Cuthbertson gives the axiom which he proposes to substitute for Euclid's 12th axiom, viz. "If one straight line be drawn in the same plane as another it cannot first recede from and then approach to the other, neither can it first approach to and then recede from the other on the same side of it." By means of this axiom and deduction G, he succeeds in proving Playfair's axiom. In other words (putting accidental defects out of the question), he succeeds in proving one axiom by assuming two. We willingly accord to this the praise of ingenuity; but we strongly suspect that few besides the author will think it an improvement on Euclid's method.

We had marked for notice our author's way of treating the subject of proportion; but our limits will not allow us to fulfil our intention. We will only say that it seems to us a feasible way of treating the subject (in the same manner as his treatment of parallels is feasible); but as to its being an improvement on Euclid's method, that is quite another matter.

XLIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 226.]

Feb. 26, 1874.—Joseph Dalton Hooker, C.B., President, in the Chair.

THE following communications were read:—

“Note on Displacement of the Solar Spectrum.” By J. H. N. Hennessey, F.R.A.S.

The following experiments were made with the (new) spectroscope (three prisms) of the Royal Society, to ascertain for this instrument the amount of displacement in the solar spectrum from change of temperature. The spectroscope was set up on a pillar within a small tent at a time of the year when the thermal range is considerable: the collimator was placed horizontal, and directed through a window in the tent to a heliostat, which was made to reflect the sun's image when required. On closing the window darkness prevailed in the tent, so that the bright sodium lines were easily obtained from a spirit-lamp. Before commencing, the slit was adjusted and the spectroscope clamped; and no movement of any kind was permitted in the instrument during the experiments. The displacement was measured by means of a micrometer in the eye-end of the telescope, readings being taken (out of curiosity) successively to both dark and bright lines, *i. e.* to $K\ 1002\cdot8 = D_r$ and $K\ 1006\cdot8 = D_v$. A verified thermometer was suspended directly over and almost touching the prisms. The meteorological observatory referred to was some fifty yards north of the tent.

Rejecting observation 5 (in the following Table) because the thermometer was evidently in advance of the prisms, we deduce

By Dark lines, displacement equal

D_r to D_v is produced by . . . $31\cdot3$ change of temperature.

By Bright lines, displacement equal

D_r to D_v is produced by . . . $29\cdot4$ „

Mean . . . 30

from which it appears that the displacement in question may not be neglected in investigations made under a considerable thermal range.

At Dehra Doon, Lat. N. 30° 20', Long. E. 78° 9', Height 2200 feet.

No. of observation.	1872. April 9.	In tent.					In Meteorological Observatory.					
		Vernier Reading.	Micrometer-readings in divisions.			Temperature near prisms.	Barometer.	Thermometers.				
			To dark lines of solar spectrum.	To bright lines from spirit-lamp.				Attached.	Air.	In sun's rays.		
				D _v .	D _r .						D _v .	D _r .
1.	h m 6 30 A.M.	} 8760, constant.	88.1	100.1	87.4	98.7	60.5	inches. 27.659	61.2	60.7	Shade 85.7	
2.	9 26 "		82.4	93.2	81.3	91.8	81.7	.725	69.9	75.4	100.0	
3.	11 55 "		76.0	87.4	74.0	85.9	92.5	.737	79.8	82.6	85.5	
4.	1 25 P.M.		73.8	85.8	71.1	83.5	95.4	.717	82.6	84.2	Shade. 84.3	
5.	4 10 "		72.9	85.2	71.0	84.0	90.7	.684				

From the above we find, mean value of space D_r to D_v by dark lines=11.7 divisions; by bright lines=11.8 divisions. Taking mean micrometer-readings for each observation, we get

	Dark lines, D _r +D _v .	Bright lines, D _r +D _v .	Temp.
	$\frac{2}{2}$	$\frac{2}{2}$	°
1.	94.1	93.1	60.5
2.	87.8	86.6	81.7
3.	81.7	80.0	92.5
4.	79.8	77.3	95.4
5.	79.1	77.5	90.7

“On White Lines in the Solar Spectrum.” By J. H. N. Hennessey, F.R.A.S.

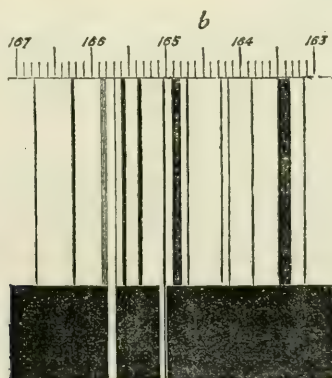
Extract from a Letter from Mr. Hennessey to Professor Stokes.

“Mussoorie, Nov. 12, 1873.

“MY DEAR SIR,—As I cannot account for what is described and drawn in enclosed, I hasten to place the same before you, intending to look for the white lines in question so soon as I move down to a lower altitude. Amongst others, no doubt Kirchhoff closely examined the region in question, without notice of the lines; and this only adds to my perplexity, unless what I see here is due (1) to altitude, or (2) is instrumental. In the latter case I cannot account for the absence of the white lines at Dehra, where I examined the spectrum generally several times; I must, however, add that without close examination and some experience, the lines might easily be passed over. But if instrumental, to what are they due? I very much regret that the old spectroscope is not available at present [it had been temporarily sent elsewhere for a special object] to enable me to verify the phenomena.”

[In the drawing sent by Mr. Hennessey, the intervals between the dark lines are coloured green, except in the place of the two white lines. To transfer this distinction to a woodcut, an additional horizontal band has been added below, in which only those parts of the drawing which are left white appear as white, while in the upper part the white of the woodcut represents the white or green, as the case may be, of the original.—G. G. S.]

Part of Solar Spectrum, drawn to Kirchhoff's scale, observed at Mussoorie, N. W. Provinces, India, Lat. N. $30^{\circ} 28'$, Long. E. $78^{\circ} 4'$; Height 6700 feet above sea (about), with the Spectroscope belonging to the Royal Society.



Note for diagram.—In course of studying the solar spectrum for atmospheric lines, with an excellent 3-prism (new) spectroscope belonging to the Royal Society, I gradually extended my search,
Phil. Mag. S. 4. Vol. 48. No. 318. Oct. 1874. X

begun at the red end, until on arrival at the region about *b* my attention was attracted by the fact that K 1657·1 by no means appeared as the strong line depicted in Kirchhoff's map, Plate II. On examining this region carefully, I was surprised to find the colourless lines shown in the diagram; these lines, from want of a more appropriate name, I shall call white lines (or spaces); they cannot absolutely be described as bright lines, yet they closely resemble threads of white floss silk held in the light. The spectroscope in use, with the most convenient highest-power eyepiece, presents images of about two thirds to seven ninths of those drawn in the diagram; the former are exaggerated by reckoning to agree with Kirchhoff's millimetre scale; it will therefore be readily understood that the white lines do not present striking objects in the spectroscope, especially about the time of sunset, when I happened first to notice them; they are best seen about noon, when their resemblance to threads of white floss silk is very close; but once seen, the lines in question can always be readily detected. So far as my instrumental means permit, the wider line extends between K 1657·1 and K 1658·3; more accurately speaking, it falls short of the latter and rather underlies the former; the narrower white line is underneath K 1650·3, sensibly more of the former appearing beyond the edge towards violet of the latter, which presents the quaint look of a black line on a white surface enclosed in a green band. These are the only white lines in the spectrum from extreme red to F; they are not bright (or reversed lines), so far as I have had opportunity to judge. Were they bright lines, the question would arise, why these alone should be reversed at 6700 feet above sea. Like the black lines the white lines grow dim and disappear with the slit opened wide. As seen here, K 1657·1 is sensibly weaker than K 1667·4, whereas Kirchhoff assigns 5 *b* to the former and only 3 *a* to the latter.

March 12.—Joseph Dalton Hooker, C.B., President, in the Chair.

The following communication was read:—

“On a New Deep-sea Thermometer.” By Henry Negretti and Joseph Warren Zambra.

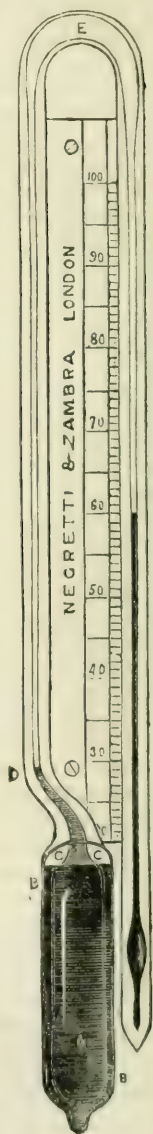
The Fellows of the Royal Society are perfectly aware of the assistance afforded by Her Majesty's Government (at the request of the Royal Society) for the purpose of deep-sea investigations, and have been made acquainted with their results by the Reports of those investigations published in the ‘Proceedings of the Royal Society’ and by the interesting work of Professor Wyville Thomson. Among other subjects, that of the temperature of the sea at various depths, and on the bottom itself, is of the greatest importance. The Fellows are also aware that for this purpose a peculiar thermometer was and is used, having its bulb protected by an outer bulb or casing, in order that its indications may not be vitiated by the pressure of the water at various depths, that pressure being about 1 ton per square inch to every 800 fathoms. This

thermometer, as regards the protection of the bulb and its non-liability to be affected by pressure, is all that can be desired; but unfortunately the only thermometer available for the purpose of registering temperature and bringing those indications to the surface is that which is commonly known as the Six's thermometer—an instrument acting by means of alcohol and mercury, and having movable indices with delicate springs of human hair tied to them. This form of instrument registers both maximum and minimum temperatures; and as an ordinary out-door thermometer it is very useful; but it is unsatisfactory for scientific purposes, and for the object for which it is now used (*viz.* the determination of deep-sea temperatures) it leaves much to be desired. Thus the alcohol and mercury are liable to get mixed in travelling, or even by merely holding the instrument in a horizontal position; the indices also are liable either to slip if too free, or to stick if too tight. A sudden jerk or concussion will also cause the instrument to give erroneous readings by lowering the indices, if the blow be downwards, or by raising them, if the blow be upwards. Besides these drawbacks, the Six's thermometer causes the observer additional anxiety on the score of inaccuracy; for, although we get a *minimum* temperature, we are by no means sure of the point where this minimum lies. Thus Professor Wyville Thomson says ('Depths of the Sea,' p. 139):—"The determination of temperature has hitherto rested chiefly upon the registration of minimum thermometers. It is obvious that the temperature registered by *minimum* thermometers sunk to the bottom of the sea, even if their registration were unaffected by the pressure, would only give the lowest temperature reached *somewhere* between top and bottom, not necessarily at the bottom itself. The temperatures at various depths might indeed (provided they nowhere increased on going deeper) be determined by a series of minimum thermometers placed at different distances along the line, though this would involve considerable difficulties. Still, the liability of the index to slip, and the probability that the indication of the thermometers would be affected by the great pressure to which they were exposed, rendered it very desirable to control their indications by an independent method." Again, at page 299, we find:—"I ought to mention that in taking the bottom temperature with the Six's thermometer the instrument simply indicates the lowest temperature to which it has been subjected; so that if the bottom water were warmer than any other stratum through which the thermometer had passed, the observations would be erroneous." Undoubtedly this would be the case in extreme latitudes, or in any spot where the temperature of the air is colder than that of the ocean. Certainly the instrument might be warmed previous to lowering; but if the coldest water should be on the surface, no reading, to be depended upon, could be obtained.

It was on reading these passages in the book above referred to that it became a matter of serious consideration with us whether a

thermometer could be constructed which could not possibly be put out of order in travelling or by incautious handling, and which should be above suspicion and perfectly trustworthy in its indications. This was no very easy task. But the instrument now submitted to the Fellows of the Royal Society seems to us to fulfil the above onerous conditions, being constructed on a plan different from that of any other self-registering thermometers, and containing as it does nothing but mercury, neither alcohol, air, nor indices. Its construction is most novel, and may be said to overthrow our previous ideas of handling delicate instruments, inasmuch as its indications are only given by upsetting the instrument. Having said this much, it will not be very difficult to guess the action of the thermometer; for it is by upsetting or throwing out the mercury from the indicating column into a reservoir at a particular moment and in a particular spot that we obtain a correct reading of the temperature at that moment and in that spot. First of all it must be observed that this instrument has a protected bulb, in order to resist pressure. This protected bulb is on the principle devised by us some sixteen years since, when we supplied a considerable number of thermometers thus protected to the Meteorological Department of the Board of Trade; and they are described by the late Admiral FitzRoy in the first Number of the ‘Meteorological Papers,’ page 55, published July 5th, 1857. Referring to the erroneous readings of all thermometers, consequent on their delicate bulbs being compressed by the great pressure of the ocean, he says:—“With a view to obviate this failing, Messrs. Negretti and Zambra undertook to make a case for the weak bulbs, which should transmit temperature, but resist pressure. Accordingly a tube of thick glass is sealed outside the delicate bulb, between which and the casing is a space all round, which is nearly filled with mercury. The small space not so filled is a vacuum, into which the mercury can be expanded, or forced by heat or mechanical compression, without doing injury to or even compressing the inner or much more delicate bulb.”

The thermometers now in use in the ‘Challenger’ Expedition are on this principle, the only difference being that the protecting chamber has



been partly filled with alcohol instead of with mercury; but that has nothing to do with the principle of the invention.

We have therefore a protected bulb thermometer, like a siphon with parallel legs, all in one piece, and having a continuous communication, as in the annexed figure. The scale of this thermometer is pivoted on a centre, and, being attached in a perpendicular position to a simple apparatus (which will be presently described), is lowered to any depth that may be desired. In its descent the thermometer acts as an ordinary instrument, the mercury rising or falling according to the temperature of the stratum through which it passes; but so soon as the descent ceases, and a reverse motion is given to the line, so as to pull the thermometer to the surface, the instrument turns once on its centre, first bulb uppermost, and afterwards bulb downwards. This causes the mercury, which was in the left-hand column, first to pass into the dilated siphon bend at the top, and thence into the right-hand tube, where it remains, indicating on a graduated scale the exact temperature at the time it was turned over. The woodcut shows the position of the mercury *after* the instrument has been thus turned on its centre. A is the bulb; B the outer coating or protecting cylinder; C is the space of rarefied air, which is reduced if the outer casing be compressed; D is a small glass plug on the principle of our Patent Maximum Thermometer, which cuts off, in the moment of turning, the mercury in the column from that of the bulb in the tube, thereby ensuring that none but the mercury in the tube can be transferred into the indicating column; E is an enlargement made in the bend so as to enable the mercury to pass quickly from one tube to another in revolving; and F is the indicating tube, or thermometer proper. In its action, as soon as the thermometer is put in motion, and immediately the tube has acquired a slightly oblique position, the mercury breaks off at the point D, runs into the curved and enlarged portion E, and eventually falls into the tube F, when this tube resumes its original perpendicular position.

The contrivance for turning the thermometer over may be described as a short length of wood or metal having attached to it a small rudder or fan; this fan is placed on a pivot in connexion with a second, and on this second pivot is fixed the thermometer. The fan or rudder points upwards in its descent through the water, and necessarily reverses its position in ascending. This simple motion or half turn of the rudder gives a whole turn to the thermometer, and has been found very effective.

Various other methods may be used for turning the thermometer, such as a simple pulley with a weight which might be released on touching the bottom, or a small vertical propeller which would revolve in passing through the water.

GEOLOGICAL SOCIETY.

[Continued from p. 230.]

December 17th, 1873.—Prof. Ramsay, F.R.S., Vice-President,
in the Chair.

The following communications were read:—

1. "Observations on some features in the Physical Geology of the Outer Himalayan region of the Upper Punjab, India." By A. B. Wynne, Esq., F.G.S.

The district of the Upper Punjab described by the author consists of crystalline, granitoid, syenitic, and schistose rocks far in among the hills, succeeded by slates and limestones, possibly of Silurian age, unconformably overlain by Triassic and perhaps older rocks, which are in their turn unconformably succeeded by a series of mutually conformable Jurassic, cretaceous, and nummulitic limestones and shaly beds. These secondary and Tertiary beds, which are chiefly limestones, are called the "Hill Limestones." Beyond these comes a zone of hills and broken plains, composed of sandstones, clays, and conglomerates, of great thickness and of Tertiary age (Eocene and Miocene), which the author calls the "Murree beds." This belt passes generally along the whole southern foot of the Himalayas, from Assam to Afghanistan. In the district described by the author it is bounded on the south by the Salt Range, beyond which stretch the deserts of the Punjab and Sind.

The outer Tertiary belt presents a gradation towards the hill character. Among the rocks of the Murree zone there are harder beds than elsewhere; limestones occasionally appear, sometimes like those of the hill-beds, and the Hill Nummulitic limestones may have alternated in their upper part with the Murree beds. The nummulitic limestones of the Salt Range, containing large Bivalves and Gasteropoda, were probably of shallow-water origin, whilst the diminutive organisms of the Hill Nummulitic limestone inhabited greater depths.

Contortion of the strata is a common feature of the country, affecting some of the newest Tertiary beds so as to place them in a vertical position, and almost everywhere throwing the rocks into folds, producing in many cases inversions of the strata.

The author compares these rocks with those of the Simla area described by Mr. Medlicott, who found there two strong unconformities, namely, between his Siwalik and Nalum, and Nalum and Subathu groups, and regarded the whole of the beds of the outer Tertiary detrital zone from the base of the Subathu group upwards as discordant to the Himalayan or Hill-series and to each other.

The junction of the newer Tertiaries with the rocks forming the higher hills of the outer Himalaya, both in the Simla area and in the outer Punjab, is marked by disturbance, distortion, and inversion or abnormal superposition in the Tertiary strata along the contact.

In the Upper Punjâb the junction follows a curved line, running nearly east and west to the north of Rawul Pindee; then describing an angle which closely follows the great bend of the Jhilm river near Mozufferabad, it runs more or less in a south-easterly direction through Kashmere towards Simla. This junction line is inseparably connected with the causation of the great mountain-chains; it shows a parallelism to the axes of the outer ranges, and is chiefly due to intensity of disturbance, the result of lateral pressure.

The author also refers to the difference existing between the geology of the outer Himalayan region and that of the Salt Range, as being similar to that which obtains between the Alpine and extra-Alpine characters of European rock-groups, and suggests that the recurrence of such similar features at such distances may indicate a connexion between the former conditions of deposition and the early history of the great chains themselves.

2. "On the mode of occurrence of Diamonds in South Africa." By E. J. Dunn, Esq. Communicated by Prof. Ramsay, F.R.S., V.P.G.S.

In this paper the author stated that the diamonds of South Africa occur in peculiar circular areas, which he regards as "pipes," which formerly constituted the connexion between molten matter below and surface volcanoes. The surrounding country consists of horizontal shales, through which these pipes ascend nearly vertically, bending *upwards* the edges of the shales at the contact. The rock occupying these pipes was regarded by the author as probably Gabbro, although in a very altered condition. Intercalated between the shale-beds there are sheets of dolerite &c.; and dykes of the same rocks also intersect the shales at frequent intervals. Within the pipes there are unaltered nodules of the same dolerite. With regard to the relation of the diamonds to the rock of the pipes in which they are found, the author stated that he thought it probable that the latter was only the agent in bringing them to the surface, a large proportion of the diamonds found consisting of fragments. At the same time he remarked that each pipe furnished diamonds of a different character from those found in other pipes.

January 7th, 1874.—Prof. Ramsay, V.P.R.S., Vice-President, in the Chair.

The following communications were read:—

1. "The Origin of some of the Lake-basins of Cumberland."—First Paper. By J. Clifton Ward, Esq., F.G.S., Assoc. R.S.M.

After referring to the fact that the question of the origin of lake-basins cannot be satisfactorily discussed unless the depth of the lakes and the heights of the mountains are brought before the mind's eye in their natural proportions, the author sketched out the physical geography of the lakes under discussion (Derwentwater,

Bassenthwaite, Buttermere, Crummock, and Loweswater), and pointed out what must have been their original size and shape before they were filled up to the extent they now are. These lakes were not moraine-dammed, but true rock-basins. The belief that the present Lake-district scenery was the result of the sculpturing of atmospheric powers, such as we see now in operation, varied by climatal changes and changes in the height of the district above the sea, was enforced, and the opinion given that the work of elaboration of the lake-country scenery has been going on ever since Carboniferous or pre-Carboniferous times. The lake-hollows represented almost the last rock-shavings removed by Nature's tools. What were the special tools producing these hollows? There being no evidence of their production by marine action or by running water, since they do not lie in synclinal troughs, nor along lines of fissuring and faulting, and cannot be supposed to be special areas of depression, it remained to see how far Professor Ramsay's theory accounted for their origin. The course of the old Borrowdale glacier was then fully traced out, and the power the numerous tributary glaciers had of helping to urge on the ice over the long extent of flat ground from Seathwaite to the lower end of Bssenthwaite Lake, commented on. The same was done with regard to the Buttermere and Ireton glacier, and the depths of the lakes, width and form of the valleys, and thickness of the ice shown by numerous transverse and longitudinal sections drawn to scale. When all the evidence was considered—the fact of the lake-hollows under examination being but long shallow troughs, the thickness of the ice which moved along the valleys in which the lakes now lie, the agreement of the deepest parts of the lakes with the points at which, from the confluence of several ice-streams and the narrowing of the valley, the onward pressure of the ice must have been greatest—the conclusion was arrived at that Prof. Ramsay's theory was fully supported by these cases, and that the immediate cause of the present lake-basins was the onward movement of the old glaciers, ploughing up their beds to this slight depth. It was pointed out that since the general form of the Buttermere and Crummock valley was that of a round-bottomed basin, as seen in transverse section, the effect of the ice was merely a slight deepening of the basin or the formation of a smaller basin of similar form at the bottom of the larger; whereas in the case of the Derwentwater and Bassenthwaite valley, which in transverse section was a wide flat-bottomed pan, the action was to form long shallow grooves at the bottom of the pan. This consideration was thought to explain the fact of the greater depth of Buttermere and Crummock than of Derwentwater and Bassenthwaite, although the size and thickness of the old glacier in the former case was probably less than in the latter. In conclusion, the author stated that he hoped to test the results obtained in these cases by bringing forward in a future paper like details of Wastwater and other lakes and mountains in the district.

2. "On the Traces of a Great Ice-sheet in the Southern part of the Lake-district and in North Wales." By D. Mackintosh, Esq., F.G.S.

In this paper the author brought forward the evidence which seems to him to establish the existence in the southern part of the Lake-district of a "valley-ignoring and ridge-concealing ice-sheet." With regard to ice-marks, he distinguished between primary striæ and those produced at a subsequent period, and stated that in the Lake-district the direction of the primary striæ generally coincides with that of the action by which *roches moutonnées* have been produced. He gave a table of the direction of ice-marks observed by him in the district under notice, and stated that about Windermere and Ambleside the general direction is nearly N.N.W., round Grasmere between N.W. and N.N.W., north-west and west of Grasmere in upland valleys and on high ridges about N. 30° W., south of Grasmere and in Great Langdale N. 35° W., and in the Coniston district a little W. of N. In many places he recognized an uphill march of the ice. He thought that the iceflow producing these marks might be anterior to the flow from south to north, of which traces are observed in the northern part of the Lake-district, and that its source was probably a vast mass of ice covering many square miles of country north of Far Easdale. The author also referred to the glaciation of North Wales, some of the marks of which, observed by him in a district south of Snowdon, seemed to him to indicate the southerly movement of a great ice-sheet capable of ignoring or crossing deep valleys. He noticed that towards the top of the pass of Llanberis there is a thin covering of drift on the S.W. side, resembling the gravelly *pinnet* of the Lake-district. He also mentioned the occurrence near Llyn Llydan of numerous mounds composed of clay, sand, and fine gravel, the stones having generally been rolled by water, and ascribed their formation to a combination of glacial and marine actions.

3. "Notes on some Lamellibranchs from the Budleigh-Salterton Pebbles." By Arthur Wyatt Edgell, Esq., F.G.S.

In this paper the author commenced by noticing the accordance between many of the pebbles of Budleigh Salterton and beds occurring on the opposite side of the channel in Brittany, and then described several species of Lamellibranchiata found in the Budleigh-Salterton pebbles. The species described were:—*Modiolopsis armorici* (Salter), *M. Lebesconti*, sp. n., *Sanguinolites*?, sp. (*contortus*?, Salter), *Aviculopecten Tromelini*, sp. n., *Pterinea retroflexa* (Hisinger) and three other species, *Palæarca*, sp., *Avicula*, sp., *Cleido-phorus*?, sp., *Lunulocardium ventricosum*, sp. n., *Ctenodonta*, sp., and *Orthonota*?, sp.

XLV. *Intelligence and Miscellaneous Articles.*

ON THE ACTION OF TWO ELEMENTS OF A CURRENT.

BY J. BERTRAND.

“TWO parallel currents attract one another when they have the same direction; they repel each other when their directions are opposite.” After enunciating this rule, Ampère believed he could immediately generalize it by extending it to the elements of the currents, to which he applies, whatever may be their relative direction, the idea of a course in the same or in opposite directions. Two currents are said to be in the same direction when they both increase their distance from the foot of the common perpendicular, or when they both approach it; in the contrary cases they have different directions. Adopting this language, it is not accurate to say that two elements having the same direction attract one the other; it is not accurate even for parallel elements. As the assertion has been reproduced in all the treatises on physics, and serves as a basis for several important explanations, I have thought it would be important to show that it is inconsistent with Ampère’s law itself, and to solve the following problem:—

Given an element of a current, to find in a point M of space the direction which must be assigned to another element in order that their mutual action may be attractive, repellent, or *nil*.

Suppose the element ds placed at the origin of the coordinates and directed along the axis of the X’s, let us seek the condition on which an element whose coordinates are x', y', z' will be without action on ds . Naming the angles formed by the two elements with the straight line which joins them θ and θ' , and the angle which they make with one another ϵ , according to the law of Ampère the condition is,

$$\cos \epsilon = \frac{3}{2} \cos \theta \cos \theta'. \quad \dots\dots\dots (1)$$

But, naming the radius vector r , and the attracting element ds' , we have

$$\cos \theta = \frac{x'}{r}, \quad \cos \theta' = \frac{dr}{ds'}, \quad \cos \epsilon = \frac{dx'}{ds'}.$$

Equation (1) becomes

$$3 \frac{x'}{r} \frac{dr}{ds'} = 2 \frac{dx'}{ds'}, \quad \dots\dots\dots (2)$$

of which the integral is

$$r^3 = A x'^2, \quad \dots\dots\dots (3)$$

the equation of a surface of revolution whose axis is the axis of X, and of which the meridian curve has for its equation, in polar co-ordinates,

$$r = A \cos^2 \theta. \quad \dots\dots\dots (4)$$

Whatever the form and direction of a current enveloping such a

s surface, the action upon an element situated at the summit and directed along the axis will be *nil*. The presence of the arbitrary constant in equation (4) permits the surface to be made to pass through any point whatever of space; and consequently there exist in each point an infinity of directions in which an element ds' may be placed so as to annul its action upon the given element ds . Those directions are all in the tangent plane to the surface found. It is readily seen that the action is a maximum when the element is normal to the surface, and that, for an element of given length and intensity, it is proportional to the cosine of the angle made with the normal. If the element ds , placed along the axis of the surface, is directed outward, every element starting from a point of the same surface and like it directed outward will be repellent, and every element directed inward will exert an attractive action.—*Comptes Rendus de l'Académie des Sciences*, vol. lxxix. pp. 141–143.

ON EARTH-CURRENTS. BY L. SCHWENDLER, ESQ.*

Mr. Schwendler said that the phenomenon of earth-currents seemed to be intimately connected with the earth-magnetism and its variations.

He would, however, point out from the beginning that though the two phenomena, “earth-magnetism” and “earth-currents,” were undoubtedly connected with each other, it was by no means established as yet that they were cause and effect, or, what certainly seems to be far more probable in the present state of knowledge on the subject, parallel effects of one and the same general but entirely unknown cause.

The three elements of the earth-magnetism, intensity, inclination, and declination, had been quantitatively and most accurately determined in almost all civilized parts of the world (Calcutta excepted) by the introduction of Gauss and Weber’s well-known system of magnetic measurements; and though the results obtained had been very general and satisfactory, establishing the most interesting facts of diurnal and secular periods of variation in the three magnetic elements, and had also been of direct practical benefit to navigation, still the physical nature of the phenomena had not been unveiled by these observations. To solve the problem, it would seem that quantitative measurements of other phenomena, directly or indirectly connected with it, were required; and it was most fortunate that at least one such phenomenon not only existed but was even susceptible of accurate measurement; he meant the “earth-currents.”

The chances of giving a true physical explanation of any phenomenon, he observed, increased in geometrical progression with the number of phenomena directly or indirectly connected with the one

* Communicated by the Author, from the Proceedings of the Asiatic Society of Bengal for June, 1874.

to be explained, supposing that they were all susceptible of accurate measurement.

In this particular case he had to deal with two such parallel phenomena—the magnetism of the earth, quantitatively ascertained for more than 40 years past, and “earth-currents,” sadly neglected.

He said he was perfectly aware why “earth-currents” had not been measured; and then, after mentioning the special purpose of his paper (*i. e.* not to start a fresh theory of the earth-magnetism with the scanty and imperfect material available, but to lay before the Society some more facts connected with its parallel phenomenon, the earth-currents in the telegraph-lines, which had been quantitatively measured during the last six years in widely different parts of the empire, Ceylon included), he proceeds as follows:—

“That it was well known that from time to time telegraph-lines, overland, underground, and submarine, were affected by what had been called ‘magnetic storms,’ *i. e.* by very strong currents passing through the wires and overpowering entirely those used for signalling, with which electrical disturbances coexisted magnetic variations far exceeding the limits generally observed when no such electrical disturbances exist, and very often accompanied in the northern (and most likely also the southern) part of the planet by vivid auroras. Now these currents observed in the telegraph-lines were ‘earth-currents.’

“For instance, on the 10th of November, 1871, and on the 4th of February, 1872, earth-currents of considerable strength had been observed in all the lines throughout India, and the submarine cables terminating on its shores. These great electrical disturbances were by no means local, but existed almost simultaneously throughout the earth, showing us a most interesting feature of our planet.

“The fact of the secular changes of the earth-magnetism occupying such a long period as about 1000 years (the principal magnetic pole moving round the astronomical pole in 1000 years) pointed most probably to a cause external to the planet. If he were allowed to follow his own imagination, he would say that earth-magnetism, its diurnal and secular variations, auroræ boreales and australes, and electrical disturbances, weak or intense, in the planet, were all due to the movement of the earth and of the heavenly bodies generally—that the great electric convulsions observed from time to time were nothing but the telegraph-signals transmitted from far distant regions to our planet, indicating great physical changes in the universe, long before, if ever, they could be felt by the more rough instruments (light, heat, and gravitation) at present the only means by which we recognize our kinship with the outer world.

“It could be, therefore, easily perceived how important it was to investigate such a phenomenon (probably of all the most widely connected) by direct measurements.

“Now if such electrical disturbances only existed by fits and

starts, as was the case during magnetic storms, it would be almost hopeless to attempt a general system of measurement. This was, however, fortunately not the case, since these earth-currents, which, during magnetic storms became so violent, seemed to exist permanently, only of very feeble strength; and it was on this subject that he would give some observed facts."

The general outline of the rest of Mr. Schwendler's communication will be best given in extracts from his paper, which will be printed in full in Part II. of the Journal.

Mr. Schwendler says:—

"The currents observed at all hours of the day and all seasons of the year, in every line throughout India, may be obviously due to many different causes acting separately or conjointly. These currents I have designated 'natural currents,' to indicate the fact of their being in the lines without any direct, or at least intentional, human agency. The causes which may produce natural currents in telegraph-lines are:—

- "1. Galvanic action between the earth-plates.
- "2. Polarization of the earth-plates by the signalling-currents.
- "3. Polarization of badly insulated points in the line.
- "4. Atmospheric electricity.
- "5. Thermo-electricity.
- "6. Inductive capacity.
- "7. Voltaic induction.
- "8. Earth-currents.

"The latter must be regarded as produced by an actual difference of potentials between the two points of our planet with which the ends of a telegraph-line are in contact.

"Surely if these 'earth-currents' do permanently exist, and, further, if they are strong enough to overpower the others, which are evidently of a much more accidental and less permanent nature, then a large number of quantitative observations, judiciously reduced and conveniently compiled, should at least show the tendency of the general law that governs them in strength and direction, leading perhaps finally to the true explanation of the earth's magnetism and the causes of its variations.

"Such were in short my reasonings when, in 1868, I was intrusted by Colonel Robinson, the Director-General of Telegraphs, with the introduction of a system of testing the lines in India; and although the practical objects of that system had nothing whatsoever to do with the solution of the problem, yet the fact that in each test measurements had to be made with positive and negative currents (for the very purpose of eliminating the influence of the natural currents) secured all the data necessary for the quantitative determination of the electromotive force in the line, to which the natural current must be considered proportional, involving only a slight additional calculation without any extra observations. To this end the necessary provisions were made and instructions issued; and in this manner more than 10,000 electromotive forces, producing the natural currents in the lines of India, have been calculated from the

tests made between 1868 and 1872, and are now at our disposal; and although the results of these numerous observations have not as yet been all analyzed, or even compiled, yet in many special cases, and for limited periods, this has been done; and from these we are justified in stating the following as facts:—

“1. All the lines in India are affected by natural currents.

“2. From more than 10,000 observations it has been established that the prevailing flow of these currents between any pair of stations is as of a copper-current from the east to the west; but which is the true direction, or that of maximum intensity, and, further, whether there is only one such direction, has not been computed as yet.

“3. The strength of the natural current in one and the same line is very variable.

“4. The direction of the natural current in one and the same line, though also variable to a certain extent, is yet far more constant than its strength, and out of a number of observations there is generally a marked preponderance of currents flowing in the same direction.

“5. The variation in strength and direction of the natural currents in parallel lines of the same length is far more uniform than might have been expected, considering the many accidental influences to which long overland lines are exposed.

“6. The prevailing direction of the natural current in any line is generally also the direction of the maximum current observed; but this is not the case invariably.

“These general facts point to one probable conclusion—namely, that ‘earth-currents’ do permanently exist in the lines of India, though they are often, and under certain circumstances even much, obscured by many other causes, of commensurate magnitude, but more unstable and accidental in character.

“For example, the two railway lines between Bombay and Madras, one of which is very perfect in insulation, while the other is quite the reverse, both exhibit a copper-current flowing permanently from Madras towards Bombay; and this fact, having been ascertained from a large number of tests, extending over a considerable period, and made from both Madras and Bombay, proves that the cause is a general one with respect to time, and that the method and place of measurement do not influence the direction of the current observed. Further, as one of the wires is used for the through traffic towards Bombay, while the other is used for the through traffic towards Madras, and as both circuits are worked with copper-currents, the natural currents, which flow in the same direction in the two wires, certainly cannot be due to the polarization of the earth-plates or of faulty places in the lines. The average electromotive force in these wires is about 4·5 Daniells; and maxima of 15 and 20 Daniells are occasionally reached.

“I consider it therefore established that ‘earth-currents’ do permanently exist in the lines of India, their general drift being from east to west, and that we should be now justified in establishing a

special system for the purpose of observing them according to a uniform plan and with improved test-methods."

Mr. Schwendler concluded by saying that, based on the facts above stated, he had proposed to the Council of the Asiatic Society to urge on Government the introduction of a system of measurement of earth-currents; that the Council had received the proposal most warmly, and had appointed Colonel Hyde, Mr. R. S. Brough, and himself to work out a practical system; and that Colonel Robinson, the Director-General of Telegraphs, had intimated his kind cooperation in the matter.

EXPERIMENTS ON THE DISSIPATION OF ELECTRICITY BY FLAMES.

BY J. W. FEWKES.

By means of an electrometer made on the principle of Sir William Thomson's quadrant, I have been able to perform a few experiments in relation to the dissipation of small quantities of electricity by different kinds of flames.

These experiments were conducted with such small quantities of electricity as could be obtained by rubbing a vulcanite plate six inches square with a catskin. The sensitiveness of the electrometer to the electricity thus formed was very great. The experiments are given below.

Experiment 1.—An alcohol lamp, carefully insulated, was connected with the electrometer. The sections of the quadrant to which it was attached were then charged by means of the vulcanite plate, the opposite sections being at the same time in connexion with the earth. The lamp was then carefully lighted. The spot of light, which had been deflected to the edge of the scale by the change, quickly returned to the zero-point, indicating a quick dissipation of the electricity by the flame.

Exp. 2.—The same conditions as those in *Exp. 1* were observed, with the exception that a Bunsen burner was substituted for the alcohol lamp. The dissipation of electricity was the same as before, and took place, as near as could be observed, at the same rate as before.

Exp. 3.—I then substituted for the Bunsen flame a very fine jet of light, obtained by passing the gas through a finely pointed glass tube. The results obtained from this experiment indicate that the rate of dissipation is in no respect related to the size of the flame.

Exp. 4.—The end of the wire connected with the quadrant was then placed so that when the gas was lighted the wire point would be in the flame. The quadrant was then charged and the gas turned on without being lighted. The spot of light had no movement, and gave no sign of any loss of electricity by the quadrant. An artificial current of air across the wire point likewise had no effect in dissipating the charge.

Exp. 5.—The end of the wire was then placed in the jet of an atomizer, the same conditions being observed as in *Exp. 1*. The

fine globules of steam and water issuing from the atomizer had no effect in dissipating the electricity of the quadrant.

I also performed two very striking experiments, which seemed to have some bearing upon this subject. The instruments used were the same as in the former experiments; and the manipulation was as follows:—

Exp. 1.—Carefully insulate a wire communicating with the electrometer, and place its point within a few inches of the flame of an insulated Bunsen burner. Let the spot of light be at the zero-point. Electrify the vulcanite plate with the catskin, and hold it at an equal distance from flame and wire point. It is very difficult under these conditions to sufficiently electrify the quadrant so as to produce any deflection of the spot of light.

Exp. 2.—Place the wire point in the flame and then hold the electrified vulcanite plate up to the flame as before. The spot of light immediately is violently deflected, indicating the presence of electricity in the quadrant. This charge, however, is soon dissipated by the flame, and the spot quickly returns to the zero-point.

These last experiments seem to indicate that the flame has a much greater attraction for the electricity of the vulcanite plate than the copper point of the wire. Hence the difficulty of charging the quadrant in the first experiment.

When, however, the wire is in direct communication with the flame, as in the second experiment, the flame and the quadrant are at the same potential, and the increase of electricity in the flame produces a corresponding deflection of the spot of light.—*Silliman's American Journal*, September 1874.

ON THE STRATIFICATION OF THE ELECTRIC LIGHT.

BY M. NEYRENEUF.

The stratifications of the electric light can be obtained in the following circumstances, which permit the production, with static electricity, of inversions of the charge as rapid as those given by the employment of the Ruhmkorff coil.

Suppose the two condensers of Holtz's machine united by a Geissler tube instead of communicating by a continuous metallic plate. Place the exciting stem of the machine so as to obtain only small sparks in very rapid succession. Two opposite currents, one of charge, the other of discharge, will then pass through the Geissler tube; and very distinct stratifications will be seen. It is necessary, in order to succeed even with very long and wide tubes, to replace the ordinary small bottles by jars of large dimensions. Those which I used had a surface of 1873 square centimetres.—*Comptes Rendus de l'Académie des Sciences*, vol. lxxix. p. 158.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

NOVEMBER 1874.

XLVI. *On the Magnetic Permeability and Maximum of Magnetism of Nickel and Cobalt.* By HENRY A. ROWLAND, C.E.*

SOME time ago a paper of mine on the magnetic permeability of iron, steel, and nickel was published in the *Philosophical Magazine* (August 1873); and the present paper is to be considered as a continuation of that one. But before proceeding to the experimental results, I should like to make a few remarks on the theory of the subject. The mathematical theory of magnetism and electricity is at present developed in two radically different manners, although the results of both methods of treatment are in entire agreement with experiment as far as we can at present see. The first is the German method; and the second is Faraday's, or the English method. When two magnets are placed near each other, we observe that there is a mutual force of attraction or repulsion between them. Now, according to the German philosophers, this action takes place at a distance without the aid of any intervening medium: they know that the action takes place, and they know the laws of that action; but there they rest content, and seek not to find how the force traverses the space between the bodies. The English philosophers, however, led by Newton, and preeminently by Faraday, have seen the absurdity of the proposition that two bodies can act upon each other across a perfectly vacant space, and have attempted to explain the action by some medium through which the force can be transmitted along what Faraday has called "lines of force."

These differences have given rise to two different ways of look-

* Communicated by Professor J. Clerk Maxwell, M.A., F.R.S.
Phil. Mag. S. 4. Vol. 48. No. 319. Nov. 1874. Y

ing upon magnetic induction. Thus if we place an electromagnet near a compass-needle, the Germans would say that the action was due in part to two causes—the attraction of the coil, and the magnetism induced in the iron by the coil. Those who hold Faraday's theory, on the other hand, would consider the substance in the helix as merely "conducting" the lines of force, so that no action would be exerted directly on the compass-needle by the coil, but the latter would only affect it in virtue of the lines of force passing along its interior, and so there could be no attraction in a perfectly vacant space.

According to the first theory, the magnetization of the iron is represented by the excess of the action of the electromagnet over that of the coil alone; while by the second, when the coil is very close around the iron, the whole action is due to the magnetization of the iron. The natural unit of magnetism to be used in the first theory is that quantity which will repel an equal quantity at a unit's distance with a unit of force; on the second it is the number of lines of force which pass through a unit of surface when that surface is placed in a unit field perpendicular to the lines of force. The first unit is 4π times the second. Now when a magnetic force of intensity \mathfrak{H}^* acts upon a magnetic substance, we shall have $\mathfrak{B} = \mathfrak{H} + 4\pi\mathfrak{I}$, in which \mathfrak{B} is the magnetization of the substance according to Faraday's theory, and is what I formerly called the magnetic field, but which I shall hereafter call, after Professor Maxwell, the magnetic induction. \mathfrak{I} is the intensity of magnetization according to the German theory, expressed in terms of the magnetic moment of the unit of volume. Now, when the substance is in the shape of an infinitely long rod placed in a magnetic field parallel to the lines of force, the ratio $\frac{\mathfrak{B}}{\mathfrak{H}} = \mu$ is called the magnetic per-

meability of the substance, and the ratio $\frac{\mathfrak{I}}{\mathfrak{H}} = \kappa$ is Neumann's coefficient of magnetization by induction. Now experiment shows that for large values of \mathfrak{H} the values of both μ and κ decrease, so that we may expect either \mathfrak{I} or both \mathfrak{B} and \mathfrak{I} to attain a maximum value.

In my former paper I assumed that \mathfrak{B} as well as \mathfrak{I} attain a maximum; but on further considering the subject I see that we

* I shall hereafter in all my papers use the notation as given in Professor Maxwell's 'Treatise on Electricity and Magnetism;' for comparison with my former paper I give the following:—

$$\begin{array}{lll} \mathfrak{B} \text{ in this paper} & = & Q \text{ in former one.} \\ \mathfrak{H} & \text{,,} & = 4\pi M \text{ ,,} \\ \mathfrak{I} & \text{,,} & = \frac{Q}{4\pi} - M \text{ ,,} \end{array}$$

have no data for determining which it is at present. If it were possible for \mathfrak{B} to attain a maximum value so that μ should approach to 0, κ would be negative, and the substance would then become diamagnetic for very high magnetizing forces*. This is not contrary to observation; for at present we lack the means of producing a sufficiently intense magnetic field to test this experimentally, at least in the case of iron. To produce this effect at ordinary temperatures, we must have a magnetic field greater than the following—for iron 175,000, for nickel 63,500, and for cobalt about 100,000 (?). These quantities are entirely beyond our reach at present, at least with any arrangement of solenoids. Thus, if we had a helix 6 inches in diameter and 3 feet long with an aperture of 1 inch diameter in the centre, a rough calculation shows that, with a battery of 350 large Bunsen cells, the magnetic field in the interior would only be 15,000 or 20,000 when the coils were arranged for the best effect. We might obtain a field of greater intensity by means of electromagnets, and one which might be sufficient for nickel; but we cannot be certain of its amount, as I know of no measurement of the field produced in this way. But our principal hope lies in heating some body and then subjecting it to a *very intense* magnetizing-force; for I have recently found, and will show presently, that *the maximum of magnetization of nickel and iron decreases as the temperature rises, at least for the two temperatures 0° C. and 220° C.* I am aware that iron and nickel have been proved to retain their magnetic properties at high temperatures, but whether they were in a field of sufficient intensity at the time cannot be determined. The experiment is at least worth trying by some one who has a magnet of great power, and who will take the trouble to measure the magnetic field of the magnet at the point where the heated nickel is placed. This could best be done by a small coil of wire, as used by Verdet.

But even if it should be proved that \mathfrak{B} does not attain a maximum, but only \mathfrak{J} , it could still be explained by Faraday's theory; for we should simply have to suppose that the magnetic induction \mathfrak{B} was composed of two parts—the first part, $4\pi\mathfrak{J}$, being due to the magnetic atoms alone, and the second, \mathfrak{H} , to those lines of force which traversed the æther between the atoms. To determine whether either of these quantities has a maximum value can probably never be done by experiment; we may be able to approach the point very nearly, but can never arrive at it, seeing that we should need an infinite magnetizing-force to do so. Hence its existence and magnitude must always be inferred from the experiments by some such process as was used

* See Maxwell's 'Treatise on Electricity and Magnetism,' art. 844.—J. C. M.

in my first paper, where the curve of permeability was continued beyond the point to which the experiments were carried. Neither does experiment up to the present time furnish any clue as to whether it is \mathfrak{B} or \mathfrak{J} which attains a maximum.

As the matter is in this undecided state, I shall hereafter in most cases calculate both \mathfrak{J} and κ as well as \mathfrak{B} and μ , as I am willing to admit that \mathfrak{J} *may* have a physical significance as well as \mathfrak{B} , even on Faraday's theory.

There is a difficulty in obtaining a good series of experiments on nickel and cobalt which does not exist in the case of iron. It is principally owing to the great change in magnetic permeability of these substances by heat, and also to their small permeability. To obtain sufficient magnetizing-force to trace out the curve of permeability to a reasonable distance, we require at least two layers of wire on the rings, and have to send through that wire a very strong current. In this way great heat is developed; and on account of there being two layers of wire it cannot escape; and the ring being thus heated, its permeability is changed. So much is this the case, that when the rings are in the air, and the strongest current circulating, the silk is soon burned off the wire; and to obviate this I have in these experiments always immersed the rings in some non-conducting liquid, such as alcohol for low temperatures and melted paraffin for high temperatures, the rings being suspended midway in the liquid to allow free circulation. But I have now reason to suspect the efficacy of this arrangement, especially in the case of the paraffin. The experiments described in this paper were made at such odd times as I could command, and the first ones were not thoroughly discussed until the series was almost completed; hence I have not been so careful to guard against this error as I shall be in the future. This can be done in the following manner—namely, by letting the current pass through the ring for only a short time. But there is a difficulty in this method, because if the current is stopped the battery will recruit, and the moment it is joined to the ring a large and rapidly decreasing current will pass which it is impossible to measure accurately. I have, however, devised the following method, which I will apply in future experiments. It is to introduce into the circuit between the tangent-galvanometer and the ring a current-changer, by which the current can be switched off from the ring into another wire of the same resistance, so that the current from the battery shall always be constant. Just before making an observation the current is turned back into the ring, a reading is taken of the tangent-galvanometer by an assistant, and immediately afterward the current is reversed and the reading taken for the induced current; the tangent-galvanometer is then

again read with the needle on the other side of the zero-point. The pressure of outside duties at present precludes me from putting this in practice. But the results which I have obtained, though probably influenced in the higher magnetizing-forces by this heating, are still so novel that they must possess value notwithstanding this defect; for they contain the only experiments yet made on the permeability of cobalt at ordinary temperatures, and of iron, nickel, and cobalt at high temperatures.

The rings of nickel and cobalt which I have used in the experiments of this paper were all turned from buttons of metal obtained by fusing under glass in a French crucible, it having been found that a Hessian crucible was very much attacked by the metal. The crucibles were in the fire three or four hours, and when taken out were very soft from the intense heat. As soon as taken out, the outside of the crucible was wet with water, so as to cool the metal rapidly and prevent crystallization; but even then the cooling inside went on very slowly. As the physical and chemical properties of these metals exercise great influence on their magnetic properties, I will give them briefly. A piece of nickel before melting was dissolved in HCl ; it gave no precipitate with H^2S , and there were no indications of either iron or cobalt. A solution of the cobalt gave no precipitate with H^2S , but contained small traces of iron and nickel. After melting the metals no tests have been made up to the present time; but it is to be expected that the metals absorbed some impurities from the crucibles. They probably did not contain any carbon. One button of each metal was obtained, from each of which two rings were turned. The cobalt was quite hard, but turned well in the lathe, long shavings of metal coming off and leaving the metal beautifully polished. The metal was slightly malleable, but finally broke with a fine granular fracture. The rings when made were slightly sonorous when struck; and the colour was of a brilliant white slightly inclined to steel-colour, but a little more red than steel. The nickel was about as hard as wrought iron, and was tough and difficult to turn in the lathe, a constant application of oil being necessary, and the turned surface was left very rough; the metal was quite malleable, but would become hard, and finally fly apart when pounded down thin if not annealed. When the rings were struck, they gave a dead sound as if made of copper. In both cases the specific gravity was considerably higher than that generally given for cast metal; but it may be that the metal to which they refer contained carbon, in which case it would be more easily melted. There is great liability to error in taking the specific gravity of these metals, because they contract so much on cooling, and unless this is carried on rapidly crystals may

form, between which, as the metal contracts, vacant spaces may be left. As the specific gravity of my rings approaches to that of the pure metals precipitated by hydrogen, I consider it evidence of their purity. The dimensions of the rings and their other constants are given below :—

Ring.	Weight <i>in vacuo</i> , in grammes.	Loss in water at 4° C., in grammes.	Specific gravity.	Mean diameter, in centimetres.
Nickel, No. I.	21·823	2·4560	8·886	3·28
Nickel, No. II.	8·887
Cobalt, No. I.	10·011	1·1435	8·7553	2·48
Cobalt, No. II.	4·681	·5346	8·7550	1·81

Ring.	Mean circumference, in centimetres.	Number of coils of wire on ring.	Coils per metre of circumference.	Area of section, in square centimetres.
Nickel, No. I.	10·304	318	3086	·2384
Nickel, No. II.
Cobalt, No. I.	7·791	243	3119	·1467
Cobalt, No. II.	5·686	158	2779	·09403

Up to the present time only the rings whose dimensions are given have been used.

The following Tables from the nickel ring No. I. leave little to be desired in point of regularity, and confirm the fact proved in my first paper, that the laws deduced for iron hold also for nickel, and also confirm the value given in my other paper for the maximum value of magnetization of nickel. But the most important thing that they show is the effect of heat upon the magnetization of nickel; and Table III. contains the first numerical data yet obtained on the effect of heat on the magnetic properties of any substance.

As all the rings were wound with two layers of wire, a slight correction was made in the value of \mathcal{B} for the lines of inductive force which passed through the air and not through the metal. In all the experiments of this paper greater care was used to obtain T than in the first paper. Each value of \mathcal{H} , \mathcal{B} , and T is the mean of four readings. In all the Tables I have left the order of the observations the same as that in which they were made, and have also put down the date, as I now have reason to suspect that the leaving of a ring in the magnetized state in which it is after an experiment will in time affect its properties to a small extent. Let me here remark that the time necessary to simply make the observations is only a very small fraction of that required to prepare for them and to afterwards discuss

them. And this, with the small amount of time at my disposal, will account for the late day at which I publish my results.

The following is the notation used, the measurements being made on that absolute system in which the metre, gramme, and second are the fundamental units.

\mathfrak{H} is the magnetizing-force acting on the metal.

\mathfrak{B} is the magnetic induction within the metal (see Maxwell's 'Treatise on Electricity and Magnetism,' arts. 400, 592, and 604).

μ is the magnetic permeability of the metal $= \frac{\mathfrak{B}}{\mathfrak{H}} = 4\pi\kappa + 1$.

T is the portion of \mathfrak{B} which disappears when the current is broken.

P is the portion of \mathfrak{B} which remains when the current is broken.

\mathfrak{I} is the intensity of magnetization $= \frac{\mathfrak{B} - \mathfrak{H}}{4\pi}$.

κ is Neumann's coefficient of induced magnetization $= \frac{\mathfrak{I}}{\mathfrak{H}}$.

TABLE I.

Cast Nickel, normal, at 15° C.

Experiments made November 29, 1873.

\mathfrak{H} .	\mathfrak{B} .	μ . Ob- served.	μ . Calcu- lated.	Error.	T.	P.	\mathfrak{I} .	κ . Ob- served.	κ . Calcu- lated.	Error.
12.84	675	52.6	46.4	-6.2	52.7	4.10	3.65	-.45
26.85	2169	80.8	80.6	-.2	1263	906	170.5	6.35	6.27	-.08
45.14	7451	165.1	166.8	1.7	2894	4557	589.3	13.06	13.08	.02
56.12	11140	198.5	199.1	.6	3788	7352	882.0	15.72	15.70	-.02
70.78	15410	217.8	217.5	-.3	5018	10392	1221	17.25	17.21	-.04
77.52	17100	220.6	220.6	.0	5454	11646	1355	17.47	17.47	0
90.76	20180	222.3	222.0	-.3	6183	13697	1599	17.61	17.60	-.01
115.4	25170	218.2	214.3	-3.9	8313	16857	1994	17.28	16.98	-.30
139.4	28540	204.7	204.3	-.4	10100	18440	2260	16.21	16.18	-.03
172.9	32460	187.8	186.6	-1.2	12530	19930	2569	14.86	14.93	.07
195.3	34630	177.3	179.1	1.8	13320	21310	2740	14.03	14.12	.09
229.5	37340	162.8	165.5	2.7	15720	21620	2953	12.87	13.02	.15
275.9	40860	148.1	146.3	-1.8	17960	22900	3230	11.71	11.46	-.25
415.2	46470	111.9	112.8	.9	22560	23910	3665	8.82	8.77	-.05
727.0	52690	72.5	72.8	.3	28020	24670	4135	5.69	5.64	-.05
1042	55680	53.4	52.8	-.6	30680	25090	4344	4.17	4.17	0
	63420	0	4940	0	

$$\mu = 222 \sin \left(\frac{\mathfrak{B} + 53 \mu + 1200}{359} \right), \quad \kappa = 17.6 \sin \left(\frac{\mathfrak{I} + 50 \kappa + 100}{28} \right).$$

TABLE II.

Cast Nickel, magnetic, at 12° C.
Experiments made December 6, 1873.

Q.	B.	μ .	T.	P.	S.	κ .
23.25	1245	53.55	97.2	4.18
47.69	7786	163.3	3095	4691	615.8	12.91
57.78	11460	198.3	3740	7720	907.3	15.70
73.43	16040	218.5	5032	11008	1270.6	17.30
88.23	19790	224.3	6554	13236	1568	17.77
107.3	23530	219.2	7620	15910	1864	17.36
153.8	30160	196.1	10940	19220	2388	15.52
206.3	35880	174.0	14030	21850	2839	13.76
296.4	41310	139.4	18390	22920	3264	11.01
421.8	46520	110.3	22520	24000	3668	8.70

TABLE III.

Cast Nickel, magnetic, at 220° C.
Experiments made December 6, 1873.

Q.	B.	μ .	T.	P.	S.	κ .
22.60	4502	199.2	2671	1831	356.4	15.77
45.06	14000	310.8	5470	8530	1111	24.65
52.96	16660	314.6	6350	10310	1322	24.96
67.42	20300	301.1	7722	12578	1602	23.88
80.69	22540	279.3	8914	13626	1787	22.15
106.4	26420	248.3	11140	15280	2094	19.68
150.8	30740	203.8	14040	16700	2434	16.14
191.0	33530	175.6	15940	17590	2653	13.89
294.8	38300	129.9	20240	18060	3024	10.26
553.6	42630	77.0	24360	18270	3348	6.05
789.8	43900	55.6	26060	17840	3431	4.345
Experiments made December 10, 1873.						
13.00	1537	118.2	109.2	9.33
22.37	4262	190.5	337.4	15.08
25.15	5337	212.2	422.7	16.81
33.19	9486	285.8	4055	5431	752.3	22.15
43.28	13570	313.6	5357	8213	1076	24.88

In Table I. are given the results for nickel at about 15° C., together with the values of μ and κ calculated from the formulæ given below the Table. We see that the coincidence is almost perfect in both cases, which thus shows that the formula which we have hitherto used for λ and μ can also be applied to κ , at least within the limit of experiments hitherto made, although it must at last depart from one or the other of the curves. The

greatest relative error is seen to be in the first line, where \mathfrak{S} is small: this does not indicate any departure from the curve, but is only due to the too small deflections of the galvanometer; and the error indicates that of only a small fraction of a division at the galvanometer.

In the calculation of μ and κ a method was used which may be of use to others in like circumstances, who have to calculate a large number of values of one variable from a function which cannot be solved with reference to that variable, but can be solved with reference to the other. Thus we have

$$\mu = \beta \sin\left(\frac{\mathfrak{B} + b\mu + \pi}{D}\right), \quad . \quad . \quad . \quad . \quad (1)$$

which can be solved with reference to \mathfrak{B} but not to μ ; for we have

$$\mathfrak{B} = D \sin^{-1}\left(\frac{\mu}{\beta}\right) - b\mu - \pi. \quad . \quad . \quad . \quad . \quad (2)$$

Suppose we have values of \mathfrak{B} , and wish to find the corresponding values of μ . We first calculate a few values of \mathfrak{B} from (2) so that we can plot the curve connecting \mathfrak{B} and μ . We then from the plot select a value of μ which we shall call μ' , as near the proper value as possible, and calculate the corresponding value of \mathfrak{B} , which we shall call \mathfrak{B}' . Our problem then is, knowing \mathfrak{B}' and μ' , to find the value of μ corresponding to \mathfrak{B} when this is nearly equal to \mathfrak{B}' . Let \mathfrak{B}' receive a small increment $\Delta\mathfrak{B}'$, so that $\mathfrak{B} = \mathfrak{B}' + \Delta\mathfrak{B}'$; then we have, from Taylor's theorem, since $\mu = \phi(\mathfrak{B}' + \Delta\mathfrak{B}')$ and $\mu' = \phi(\mathfrak{B}')$,

$$\mu = \mu' + \frac{d\mu'}{d\mathfrak{B}'} (\Delta\mathfrak{B}') + \frac{1}{2} \frac{d^2\mu'}{d\mathfrak{B}'^2} (\Delta\mathfrak{B}')^2 + \&c.$$

Remembering that the constants in (1) refer to degrees of arc and not to the absolute value of the arc, we have

$$\mu = \mu' + \frac{\mathfrak{B} - \mathfrak{B}'}{57.3 \frac{D}{\beta} \frac{1}{\sqrt{1 - \frac{\mu'^2}{\beta^2}}} - b} + \&c.,$$

which is in the most convenient form for calculation by means of Barlow's Tables of squares, &c., and is very easy to apply, being far easier than the method of successive approximation.

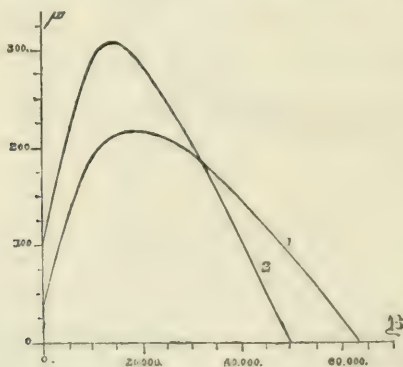
On comparing the magnetic curve Table II. with the normal curve Table I., we see that the magnetic curve of nickel bears the same relation to the normal curve as we have already found for iron; that is, the magnetic curve falls below the normal curve for all points before the vertex, but afterwards the two coincide.

Hence we see that at ordinary temperatures the magnetic pro-

properties of nickel are a complete reproduction of those of iron on a smaller scale. But when we come to study the effect of temperature we shall find a remarkable difference, and shall find nickel to be much more susceptible than iron to the influence of heat.

In Table III. we have experiments on the permeability of nickel at a high temperature, the ring being maintained at 220°C . by being placed in a bath of melted paraffin: in this bath the silk covering of the wire remained quite perfect, but after many hours became somewhat weak. After completing the experiments on this and the cobalt rings, on unwinding some of them I found the outside layer quite perfect; but, especially in the smallest ring, the silk on the inside layer was much weaker, although the insulation was still perfect when the wire was in place. I can only account for this by the electric current generating heat in the wire, which was unable to pass outward because of the outside layer and also of the pieces of paper which were used to separate the layers of wire; hence the ring at high magnetizing-powers must have been at a somewhat higher temperature than the bath, to an amount which it is impossible to estimate. It is probable that it was not very great, however; for at this high temperature continued for hours it requires but little increase of heat to finally destroy the silk. We can, however, tell the direction of the error.

We see, on comparing Tables I. and II. with Table III., the great effect of heat on the magnetic properties of nickel. We see that for low magnetization the permeability is greatly *increased*, which is just opposite to what we might expect; but on plotting the curve we also notice the equally remarkable fact,

1. Curve at 15°C .2. Curve at 220°C .

that the maximum of magnetization is *decreased* from $B=63400$ or $H=4940$ to $B=49000$ or $H=3800$. This curious result is shown in the annexed figure, where we see that for low magne-

tizing-forces μ is increased to about three or four times its value at 15° C., and the maximum value of μ is increased from 222 to 315. When \mathcal{B} has a value of 32,000, μ is not affected by this change of temperature, seeing that the two curves coincide; but above that point μ is less at 220° C. than at 15° C. In other words, *if nickel is heated from 15° C. to 220° C., the magnetization of nickel will increase if the magnetizing-force is small, but will decrease if it is large.* It is impossible to say at present whether increase of temperature above 220° will always produce effects in the same direction as below it or not.

These remarkable effects of heat, it seems to me, will, when followed out, lead to the discovery of most important connexions between heat and magnetism, and will finally result in giving us much more light upon the nature of heat and magnetism, and that equally important question of what is a molecule. To accomplish this we must obtain a series of curves for the same ring between as wide limits of temperature as possible. We must then plot our results in a suitable manner; and from the curves thus formed we can find what would probably happen if the temperature were lowered to the absolute zero, or were increased to the point at which nickel is said to lose its magnetism. In such inquiries as these the graphical method is almost invaluable, and little can be expected without its aid.

In applying the formula to this curve, we do not find so good an agreement as at the lower temperature. I do not consider this conclusive that the formula will not agree with observation at this temperature; for I have noticed that the curves of different specimens of iron and nickel seem to vary within a minute range, not only in their elements but also in their form. This might perhaps be accounted for by some small want of homogeneity, as in the case of burning in iron and nickel; but at present the fact remains without an explanation. But the amount of the deviation is in all cases very small when all the precautions are taken to insure good results. The nature of the deviation is in this case as follows: when the constants in the formula are chosen to agree with the observed curve at the vertex and at the two ends, then the observed curve falls slightly below the curve of the formula at nearly all other points. In a curve plotted about 5 inches high and broad, the greatest distance between the two curves is only about $\frac{1}{12}$ of an inch, and could be much reduced by changing the constants. For the benefit of those who wish to study this deviation, I have calculated the following values, which will give the curve touching the vertex and the two ends of the observed curve of Table III. They are to be used by plotting in connexion with that Table.

κ .	\mathfrak{S} .	
0	-140	3802
12.5	205	2833
18.75	455	2269
22.5	703	1835
25	1206	

$$\kappa = 25 \sin \frac{\mathfrak{S} + 25 \kappa + 140}{21.9}.$$

I have not as yet obtained a complete curve of iron at a high temperature; but as far as I have tried, it does not seem to be affected much, at least for high magnetizing-powers. I have, however, found that the maximum of magnetization of iron decreases about 2 per cent. by a rise of temperature from 15° C. to 222° C., while that of nickel decreases 22.7 per cent.

The experiments which I have made with cobalt do not seem to be so satisfactory as those made with nickel and iron. There are some things about them which I cannot yet explain; but as they are the only exact experiments yet made on cobalt, they must possess at least a transient value. The difficulties of getting a good cobalt-curve are manifold, and are due to the following properties—(1) its small permeability, (2) its sensitiveness to temperature, and (3) its property of having its permeability increased by rise of temperature at all magnetizing-powers within the limits of experiment. The following are the results with No. I.:—

TABLE IV.
Cast Cobalt, normal, at 5° C.
Experiments made November 27, 1873.

\mathfrak{S} .	\mathfrak{B} .	μ .	T.	P.	\mathfrak{S} .	κ . Ob- served.	κ . Calcu- lated.	Error.
49.33	4303	87.24	3702	601	338.5	6.86	6.75	-.11
58.83	5608	95.32	4526	1082	441.6	7.51	7.44	-.07
76.47	8409	109.95	6175	2234	663.1	8.67	8.79	.12
93.15	11623	124.8	7826	3797	917.5	9.85	9.81	-.04
113.0	14993	132.7	9805	5188	1193.1	10.48	10.44	-.04
129.3	17439	134.9	10580	6859	1387.8	10.66	10.72	.06
159.4	22309	140.0	14090	8219	1775.3	11.06	11.00	-.06
189.0	26769	141.6	16260	10509	2130.3	11.19	10.97	-.22
219.6	30580	139.3	18200	12380	2433.5	11.01	10.83	-.18
264.7	35525	134.2	21120	14405	2827.0	10.60	10.50	-.10
351.1	43421	123.7	25670	17751	3455.0	9.76	9.73	-.03
400.0	46640	116.6	27830	18810	3711.5	9.20	9.34	.14
552.1	55410	100.4	34090	21320	4409.0	7.91	8.16	.25
732.1	63400	86.6	39850	23550	5045.0	6.81	6.93	.12
999.8	71800	71.8	47310	24490	5714.0	5.63	5.55	-.08
1471	80770	54.9	55870	24900	6430.0	4.29	3.98	-.31
					8160	0	

$$\kappa = 11 \sin \frac{\mathfrak{S} + 190 \kappa + 120}{46}.$$

TABLE V.

Cast Cobalt, magnetic, at -5°C .

Experiments made November 28, 1873.

\mathfrak{S} .	\mathfrak{B} .	μ .	T.	P.	\mathfrak{Z} .	κ .
48.47	3702	76.37	3287	415	290.8	6.00
76.74	7254	94.54	5760	1494	571.1	7.44
112.8	14370	127.5	9388	4982	1134.5	10.06
167.6	24130	144.0	14490	9640	1907	11.38
264.2	35860	135.7	20420	15410	2833	10.72
539.9	53940	99.91	33010	20930	4249	7.87
1473	80760	54.84	55920	24840	6310	4.28

TABLE VI.

Cast Cobalt, magnetic, at 230°C .

Experiments made February 3, 1874.

\mathfrak{S} .	\mathfrak{B} .	μ .	T.	P.	\mathfrak{Z} .	κ .
13.34	1357	101.8	1165	192	107	8.02
25.67	2916	113.6	2662	254	230	8.96
38.55	4940	128.2	4397	543	390	10.12
55.56	9400	169.1	7440	1960	743.5	13.38
75.16	15800	210.2	10050	5750	1143	16.65
101.4	23920	235.9	14260	9660	1895	18.70
132.7	31260	235.5	17710	13550	2475	18.66
172.9	38060	220.2	21820	16240	3015	17.44
281.8	52520	186.4	31160	21360	4174	14.76
393.6	63430	161.2	39070	24360	5039	12.75
702.9	82070	117.0	54920	27150	6515	9.27
989.3	95600	96.63	66750	28850	7584	7.67
1282	106200	82.87	75820	30380	8422	6.57

From Table IV. we see that at ordinary temperatures cobalt does not offer any exception to the general law for the other magnetic metals—that as the magnetization increases, the magnetic permeability first increases and then decreases. We also see that the results satisfy to a considerable degree of accuracy the equation which I have used for the other magnetic metals. The departure from the equation is of exactly the nature that can be accounted for in either of two ways—either by the heating of the ring by the current for the higher magnetizing-forces, or by some want of homogeneity in the ring. According to the first explanation, the maximum of magnetization at 0°C . will be somewhat lower than the curve indicates; but by the second it must be higher. I, however, incline to the first, that it is due to heating, for two reasons: first, it is sufficient; and secondly, the smaller cobalt ring gives about the same maximum

as this. Hence we may take as the provisional value of the maximum of magnetization of cobalt in round numbers $\mathfrak{J} = 8000$, or $\mathfrak{B} = 100,000$.

We also see from Table IV. that, at least in this case, the permeability of cobalt is less than that of nickel, though we could without doubt select specimens of cobalt which should have this quality higher than a given specimen of nickel. The formula at the foot of the Table also shows, by the increased value of the coefficient of κ in the right-hand member, that the diameter of the curve is much less inclined to the axis of \mathfrak{J} in this case than in the case of nickel or iron. In this respect the three metals at present stand in the following order—cobalt, nickel, iron. This is the inverse order also of their permeability; but at present I have not found any law connecting these two, and doubt if any *exact* relation exists, though as a general rule the value of the constant is greater in those curves where the permeability is least.

In a short abstract in the 'Telegraphic Journal,' April 1, 1874, of a memoir by M. Stefan, it is stated "that the resistance of iron and nickel to magnetization is at first very great, then decreases to a minimum value, which is reached when the induced magnetic moment is become a third of its maximum." This will do for a very rough approximation, but is not accurate, as will be seen from the following Table of this ratio from my own experiments:—

Experiments published in August 1873.					
Iron. Tables I. and. II.	Iron. Table III.	Bessemer steel. Table IV.	Iron. Table V.	Nickel. Table VI.	Steel. Table VII.
$\frac{1}{3.02}$	$\frac{1}{2.64}$	$\frac{1}{2.65}$	$\frac{1}{2.68}$	$\frac{1}{3.15}$	$\frac{1}{2.46}$
Experiments of present paper.					
Nickel. Tables I. and II.	Nickel. Table III.		Cobalt. Tables IV. and V.		
$\frac{1}{3.23}$	$\frac{1}{3.14}$		$\frac{1}{4.2}$		

The average of these is, if we include Bessemer steel with the iron, as it is more iron than steel:—

$$\text{Iron, } \frac{1}{2.75} = \frac{4}{11}; \quad \text{Nickel, } \frac{1}{3.17}; \quad \text{Cobalt, } \frac{1}{4.2}.$$

Hence the place of greatest permeability will vary with the kind of metal. From these, however, we can approximate to the value of b in the formula; for we have

$$\text{for Iron, } b = \frac{27,000}{\beta}; \quad \text{for Nickel, } b = \frac{11,000}{\beta};$$

$$\text{for Cobalt, } b = 26,000.$$

In Table V. we have the results for cobalt in the magnetic state. We here find the same effect of magnetization as we have before found for iron and nickel.

In Table VI. we have results for cobalt at a high temperature, and see how greatly the permeability is increased by rise of temperature, this being for the vertex of the curve about 70 per cent. But on plotting the curve I was much surprised to find an entire departure from that regularity which I had before found in all curves taken from iron and nickel when the metal was homogeneous. At present I am not able to account for this, and especially for the fact that one of the measurements of \mathcal{B} is higher than that which we have taken for the maximum of magnetization, at, however, a lower temperature. The curve is exactly of the same nature as that which I have before found for a piece of nickel which had been rendered unhomogeneous by heating red-hot, and thus burning the outside. The smaller cobalt ring gives a curve of the same general shape as this, but has the top more rounded. I will not attempt without fresh experiments to explain these facts, but will simply offer the following explanations, some one of which may be true. First, it may be due to want of homogeneity in the ring; but it seems as if this should have affected the curve of Table IV. more. Secondly, it may be at least partly due to the rise in temperature of the ring at high magnetizing-powers; and indeed we know that this must be greater in paraffin than in alcohol for several reasons: there is about twice as much heat generated in copper wire at 230°C. as at 0° with the same current; and this heat will not be conducted off so fast in paraffin as in alcohol, on account of its circulating with less freedom; it probably has less specific heat also. Thirdly, it may be due to some property of cobalt, by which its permeability and maximum of magnetization are increased by heat and the curve changed.

The experiments made with the small ring confirm those made with the large one as far as they go; but as it was so small, they do not possess the weight due to those with the larger one. But, curious as it may seem, although they were turned from the same button side by side, yet the permeability of the larger is about 45 per cent. greater than that of the smaller. I have satisfied myself that this is due to no error in

experiment, but illustrates what extremely small changes will affect the permeability of any metal.

We have now completed the discussion of the results as far as they refer to the magnetic permeability, leaving the discussion of the temporary and permanent or residual magnetism to the future, although these latter, when discussed, will throw great light upon the nature of the coercive force in steel and other metals. The whole subject seems to be a most fruitful one, and I can hardly understand why it has been so much neglected. It may have been that a simple method of experiment was not known; but if so, I believe that my method will be found both accurate and simple, though it may be modified to suit the circumstances. Professor Maxwell has suggested to me that it would be better to use rods of great length than rings, because that in a ring we can never determine its actual magnetization, but must always content ourselves with measuring the *change* on reversing or breaking the current. This is an important remark, because it has been found by MM. Marianini and Jamin, and was noticed independently by myself in some unpublished experiments of 1870, that a bar of steel which has lain for some time magnetized in one direction will afterwards be more easily magnetized in that direction than in the other. This fact could not have been discovered from a ring; and indeed if a ring got a one-sided magnetism in any way we might never know it, and yet it might affect our results, as indeed we have already seen in the case of the magnetic curve. But at the same time I think that greater errors would result from using long bars. I have tried one of iron 3 feet long and $\frac{1}{4}$ inch diameter; and the effect of the length was still apparent, although the ratio of length to diameter was 144. To get exact results it would probably have to be several times this for the given specimen of iron, and would of course have to be greater for a piece of iron having greater permeability. This rod must be turned and must be homogeneous throughout—conditions which it would be very difficult to fulfil, and which would be impossible in the case of nickel and cobalt. We might indeed use ellipsoids of very elongated form; and this would probably be the best of all, as the mathematical theory of this case is complete, and it is one of the few where the magnetization is uniform, and which consequently will still hold, although the permeability may vary with the amount of magnetization. This form will, of course, satisfy Professor Maxwell's objection.

The method of the ring introduces a small error which has never yet been considered, and which will affect Dr. Stoletow's results as well as mine. The number of lines of induction passing across the circular section of a ring-magnet we have seen

to be

$$Q_1 = 4\pi n' i \int_{-R}^{+R} \mu \frac{\sqrt{R^2 - x^2}}{a - x} dx,$$

in which a is the mean radius of the ring, R the radius of the section, n' the number of coils in the helix, and i the intensity of the current. Now in integrating this before, I assumed that μ was a constant throughout the section of the ring: now we have found that μ is a function of the magnetization, and hence a function of the magnetizing-force; but the latter varies in different parts of the section, and hence μ must vary. But the correction will be small, because the average value will be nearly the same as if it were a constant. We may estimate the correction in the following manner. Let μ and \mathfrak{H} be the values of those quantities at any point in the section of the ring, μ' and \mathfrak{H}' the values at the centre of the section, and μ_1 and \mathfrak{H}_1 the observed values. Then, by Taylor's theorem,

$$\mu = \mu' + \frac{d\mu'}{d\mathfrak{H}'} (\mathfrak{H} - \mathfrak{H}') + \frac{1}{2} \frac{d^2\mu'}{d\mathfrak{H}'^2} (\mathfrak{H} - \mathfrak{H}')^2 + \&c.$$

But $\mathfrak{H} = \frac{2\pi n' i}{a - x}$ and $\mathfrak{H}' = \frac{2\pi n' i}{a}$, and so we have

$$Q_1 = \pi R^2 \mathfrak{H}' \mu' \left\{ 1 + \frac{1}{4} \frac{R^2}{a^2} + \&c. + \frac{\mathfrak{H}'}{2\mu'} \frac{d\mu'}{d\mathfrak{H}'} \left(\frac{R^2}{a^2} + \frac{R^4}{a^4} + \&c. \right) + \frac{\mathfrak{H}'^2}{8\mu'} \frac{d^2\mu'}{d\mathfrak{H}'^2} \left(\frac{R^2}{a^2} + 3 \frac{R^4}{a^4} + \&c. \right) + \&c. \right\}.$$

But in my Tables I have already calculated

$$\mu_1 = \frac{Q_1}{\pi R^2 \mathfrak{H}' \left(1 + \frac{1}{4} \frac{R^2}{a^2} + \&c. \right)};$$

and as μ_1 is very nearly equal to μ' , and \mathfrak{H}_1 to \mathfrak{H}' , we have approximately

$$\mu' = \mu_1 \left\{ 1 - \frac{\mathfrak{H}_1}{2\mu_1} \frac{d\mu_1}{d\mathfrak{H}_1} \left(\frac{R^2}{a^2} + \frac{3}{4} \frac{R^4}{a^4} + \&c. \right) - \frac{\mathfrak{H}_1^2}{8\mu_1} \frac{d^2\mu_1}{d\mathfrak{H}_1^2} \left(\frac{R^2}{a^2} + \frac{11}{4} \frac{R^4}{a^4} + \&c. \right) - \&c. \right\},$$

which will give the value of μ' corresponding to Q' and \mathfrak{H}' . Hence the correct values of the quantities will be μ' , \mathfrak{H}' , and $\mathfrak{B}' = \mathfrak{H}'\mu'$.

The quantities $\frac{d\mu_1}{d\mathfrak{H}_1}$ and $\frac{d^2\mu_1}{d\mathfrak{H}_1^2}$ can be obtained either by mea-

suring a plot of the curve, or from the empirical equation

$$\mu = B \sin \frac{\mu \mathfrak{H} + b\mu + \pi}{D},$$

when we know the values of the constants. In this case

$$\frac{d\mu}{d\mathfrak{H}_l} = \pm \frac{\mu_l \sqrt{B^2 - \mu_l^2}}{C},$$

$$\frac{d^2\mu_l}{d\mathfrak{H}_l^2} = \frac{C\mu_l(2B^2 - 3\mu_l^2) \mp \mu_l^3(\mathfrak{H}_l + b) \sqrt{B^2 - \mu_l^2}}{C^3},$$

in which

$$C = 57.3D \mp (\mathfrak{H} + b) \sqrt{B^2 - \mu_l^2}.$$

In all these the upper signs are to be taken for all values of \mathfrak{H}_l less than $\frac{90D - bB - \pi}{B}$, and the lower signs for greater values.

On applying these formulæ to the observations, I have found that the corrections will in no way influence my conclusions, being always very small; but at the same time the calculation shows that it would be well to diminish the ratio $\frac{R}{a}$ as much as possible. In all my rings this ratio did not depart very much from $\frac{1}{6.3}$; but I would advise future experimenters to take it at least as small as $\frac{1}{10}$: the amount of correction will be very nearly proportional to the square of $\frac{R}{a}$.

Summary.

The following laws have been established entirely by my own experiments, though in that part of (2) which refers to iron I have been anticipated in the publication by Dr. Stoletow (Phil. Mag. Jan. 1873). When any measurements are given, they are on the metre, gramme, second system.

(1) Iron, nickel, and cobalt, in their magnetic properties at ordinary temperatures, differ from each other only in the quantity of those properties and not in the quality.

(2) As the magnetizing-force is increased from 0 upwards, the resistance of iron, nickel, and cobalt to magnetization decreases until a minimum is reached, and after that increases indefinitely. This minimum is reached when the metal has attained a magnetization of from .24 to .38 of the maximum of magnetization of the given metal.

(3) The curve showing the relation between the magnetization and the magnetic permeability, or Neumann's coefficient, is of such a form that a diameter can be drawn bisecting chords parallel to the axis of \mathfrak{B} , and is of very nearly the form given by the equation

$$\mu = \mathfrak{B} \sin \frac{\mathfrak{B} + b\mu + \pi}{D},$$

where \mathfrak{B} , b , and D are constants, μ is the ratio of the magnetization to the magnetizing-force in an infinitely long bar, and \mathfrak{B} is the amount of magnetization.

(4) If a metal is permanently magnetized, its resistance to change of magnetism is *greater* for low magnetizing-powers than when it is in the normal state, but is the same for high magnetizing-powers. This applies to the permanent state finally attained after several reversals of magnetizing-force; but if we strongly magnetize a bar in one direction and then afterwards apply a weak magnetizing-force in the opposite direction, the change of magnetization will be very great.

(5) The resistances of nickel and cobalt to magnetization vary with the temperature; but whether it is increased or not in nickel depends upon the amount of magnetization: for a moderate amount of magnetization it *decreases* with rise of temperature very rapidly; but if the magnetization is high the resistance is *increased*. In cobalt it apparently always decreased, whatever the magnetization. The resistance of iron to magnetization is not much affected by the temperature.

(6) The resistance of any specimen of metal to magnetization depends on the kind of metal, on the quality of the metal, on the amount of permanent magnetization, on the temperature, and on the total amount of magnetization, and, in at least iron and nickel, decreases very much on careful annealing. The maximum of magnetization depends on the kind of metal and on the temperature.

(7) Iron, nickel, and cobalt all probably have a maximum of magnetization, though its existence can never be entirely established by experiment, and must always be a matter of inference; but if one exists, the values must be nearly as follows at ordinary temperatures. Iron when $\mathfrak{B}=175,000$ or when $\mathfrak{I}=139,000$; nickel when $\mathfrak{B}=63,400$ or when $\mathfrak{I}=49.10$; cobalt when $\mathfrak{B}=100,000(?)$ or when $\mathfrak{I}=8000(?)$.

(8) The maximum of magnetization of iron and nickel decreases with rise of temperature, at least between 10° C. and 220° C., the first very slowly and the second very rapidly. At 220° C. the maximum for iron is when $\mathfrak{B}=172,000$ and $\mathfrak{I}=13,600$, and for nickel when $\mathfrak{B}=49,000$ and $\mathfrak{I}=3800$.

The laws which govern temporary and residual magnetism, except so far as they have been hitherto given, I leave for the future, when I shall have time for further experiment on the subject to develop some points which are not yet quite clear.

Troy, New York, U.S.A.,
April 1874.

Errata in Vol. 46.

Page 144, equation (2), first term of second member, for $(\sqrt{RR'} + s')$
read $(\sqrt{RR'} - s')$.

— 144, second line of note, for vol. iv. p. 669 read vol. iii. p. 77.

— 148, line 24 from top, for 14.51 read 14.15.

— 156, line 7 from bottom, for $\frac{Q^2}{4\pi}$ read $\frac{Q^2}{8\pi}$.

8(1874) XLVII. *Experiments on Electrical Vibrations*.
By ANTHUR SCHUSTER, Ph.D.*

I. *Introductory.*

IN a previous paper† I described a curious effect of electrical vibrations on the galvanometer-needle. The effect could only be explained by assuming a different conductivity in opposite directions. This unilateral conductivity, as it was called, is never a stable phenomenon, but generally disappears very soon by suitable manipulations. The condition of the circuit in which no unilateral conductivity appeared was called the normal condition of the circuit. The experiments which are described in this paper were all made when the wire was in its normal condition, and when, therefore, a magnet rotating in a coil of wires connected with the galvanometer produced no effect on the galvanometer-needle.

It occurred to me to send a permanent current through the galvanometer, in addition to the electrical vibrations induced by the rotating magnet. From previously known facts it would be expected that the electrical vibrations would counterbalance each other independently of any permanent current going through the galvanometer, and therefore that the permanent current would produce the same deflection whether the magnet is rotating or not. This, however, is not the case, but the rotation of the magnet always increases the deflection of the permanent current.

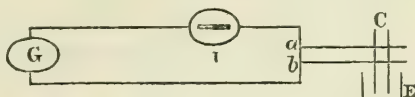
II. *Description of Experiments.*

The instruments used were the same as those described in my

* Communicated by the Author.

† Phil. Mag. vol. xlviii. p. 251.

paper "On Unilateral Conductivity." The arrangement is shown by the following diagram:—



G is a galvanometer, the resistance of which was 2477 mercury units; I is the induction-coil within which the rotating magnet is placed; C is a commutator; E a Daniell's cell.

It is seen by the diagram that only a small part of the permanent current passed through the galvanometer, as the resistance of the wire *ab* was very small compared with the resistance of the galvanometer. The experiments were conducted in the following way:—

1. The rotating magnet was first left fixed in a certain position, and the first deflection produced by E was measured. It was found that this deflection was the same in whatever position the magnet was fixed; and we may therefore conclude that the reaction on the current of the magnetization produced by the current was too small to be observed. The magnet was set in rotation now after contact had been broken at *c*; and when the needle in G had come to rest again, which was done in a very short time (the galvanometer having a strong logarithmic decrement), the first deflection produced by E was measured. It was found that the first deflection was always larger when the magnet rotated. The following is a series of observations which I take at random out of my laboratory book.

	First deflection, in scale-divisions.
Magnet quiet	322
" rotating	333
" quiet	321
" rotating	327
" quiet	311
" rotating	321
" quiet	307
" rotating	320
" quiet	305
Mean for magnet rotating	325.25
Mean for magnet quiet	313.20
Difference	12.05

2. By altering the resistance of the wire *ab*, I could alter the strength of the permanent current without sensibly affecting the

strength of the electrical vibrations. The following Table shows that the difference in the first deflection which is observed according as the magnet rotates or not, is sensibly proportional to the strength of the permanent current. The first column gives the first deflection observed when the magnet was not rotating; the second column gives the difference in the deflection when the magnet rotated; and the third column gives the ratio of the numbers given in the first two columns. The numbers given in the third column are as nearly constant as could be expected. The chief error of observation is caused by the difficulty of keeping the rotation of the magnet constant for a sufficient length of time.

First deflection (magnet quiet).	Difference in the first deflection.	Ratio.
336	18.6	0.055
285	16.1	0.056
230	11.4	0.050
168	9.8	0.052
113	7.1	0.050
67	4.1	0.061

3. By altering the resistance of the wire $b G I a$ and that of the wire $a b$ at the same time, we can alter the strength of the electrical vibrations, without altering the strength of the permanent current produced by the electromotive force E . The following Table shows that the effect decreases very rapidly as the strength of the electrical vibrations decreases. The first column gives the resistance R introduced into the circuit $b G I a$; the second column gives the ratio n of the difference in the first deflection observed according as the magnet rotated or did not rotate, to the first deflection observed when the magnet was quiet.

R .	n .
2000	0.018
1000	0.018
500	0.030
0	0.016

4. Another series of experiments was made with a galvanometer the resistance of which was only a few units. The first deflection produced by the electromotive force in E was about the same as had been observed with the other galvanometer, because, although the delicacy of the galvanometer was much smaller, a greater part of the current passed through the galvanometer, the resistance $b I G a$ being now a great deal smaller

than it had been before. The electrical vibrations were, of course, much stronger now; but each of them taken separately would have produced about the same deflection of the galvanometer. The galvanometer had no damping-arrangement. The needle, therefore, never was entirely at rest; and, accordingly, the first deflection could not be measured. Five successive elongations were taken, and the position of rest calculated from these elongations. As the observations necessarily extended over a much greater length of time, they were less accurate. The result is given in the following Table. The first column gives the first deflection observed when the magnet was not rotating; the second column gives the ratio n of the difference in the first deflections to the first deflection observed when the magnet was not rotating.

First deflection observed when magnet was quiet.	n .
248	0.076
205	0.069
164	0.053
124	0.070
99	0.046

With the exception of the last observation but one, therefore, the effect decreases more rapidly than the current.

III. *Discussion of Experiments.*

We shall have to discuss now whether these experiments can be explained in any simple way or by any known causes. At first sight three possible explanations suggest themselves; and we have to see whether one of them, or several of them taken together account satisfactorily for the different experiments, before we assume that we have to do with a new set of phenomena. We might assume:—

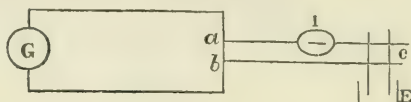
1. That the electrical vibrations affect the electromotive force of the battery;

2. That the electrical vibrations affect the magnetism of the galvanometer-needle so as to increase the deflection of the permanent current;

3. That the permanent current affects the magnetism of the rotating magnet in such a way as to increase its momentum while the current in one direction is passing, and to decrease it while the current in the opposite direction is passing. The two opposite currents induced by the magnet would therefore not counterbalance each other, but one would be stronger than the other.

We shall discuss these explanations in their order.

1. The explanation given under 1 does not at first sight seem unlikely. We do not know how these electrical vibrations affect the polarization of the battery. They might decrease it for all we know, and thereby cause the phenomena which we have observed. In order to settle this question experimentally, the apparatus was disposed as indicated by the following diagram : --



The inductor *I* was taken out of the galvanometer circuit and put into the circuit of the battery. The strength of the electrical vibrations passing through the battery was therefore now about 1700 times as great as before. We should expect the effect to be much stronger now if it were caused by any effect of the electrical vibrations on the battery. As, however, the deflection was exactly the same now whether the magnet rotated or not, we may safely conclude that this explanation is not the correct one. I do not say it is proved by these experiments that electrical vibrations do not affect the polarization of the battery. On the contrary, I think it would be important to make more careful experiments on the subject ; but an inspection of the diagram will show that a small alteration in the electromotive force would not be apparent in these experiments. All we can say, therefore, is, that the change in the electromotive force of the battery, if it exists, is not sufficiently large to affect the deflection of the permanent current if the apparatus is disposed as indicated in the above diagrams. This, of course, is sufficient for our purpose.

2. We have now to see whether the electrical vibrations can affect the galvanometer-needle in such a way as to produce the effect which was observed. No other cause than the bilateral deflection, discovered by Poggendorff, and mentioned in my paper on unilateral conductivity, is known which could affect the experiments. It can be shown that the bilateral deflection is no stable phenomenon ; that is to say, either it does not influence the position of rest of the needle, or it places the needle parallel to the axis of the galvanometer-coil. Experiments were therefore made in which, instead of taking the first deflection as a measure of the strength of the current, the permanent deflection was measured. It was found that when the needle had come to rest while the permanent current was passing, the position of rest was altered by the rotation of the magnet.

Additional experiments to prove the impossibility of accounting for the effect by bilateral deflection were made. The needle was deflected by permanent magnets placed near the galvanometer. The relative position of the galvanometer-needle to the axis of the coil was therefore changed; and this was done in such a way that bilateral deflection, instead of increasing the deflection, would tend to diminish it. As, however, in this case the deflection was just as much increased by the rotation of the magnet as before, I consider it proved beyond doubt that no change of magnetization of the galvanometer-needle can have caused the effect.

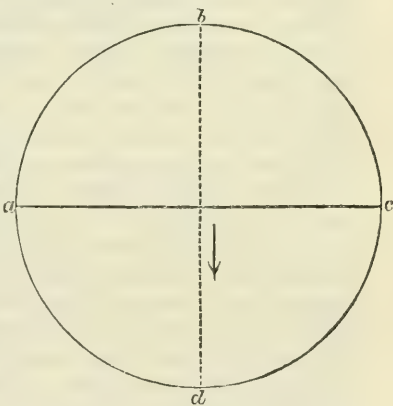
3. It remains for us to consider in detail what effect the permanent current has on the magnetism of the rotating magnet.

Let ac be the axis of the coil within which the magnet rotates. The wires of the coil

are therefore parallel to bd .

Suppose the current passes in the direction indicated by the arrow-head in that part of the coil which is above the plane of the paper. The permanent current would in that case increase the strength of the rotating magnet while the north pole moves from b to d , the rotation being in the direction of the movement of the hands of a watch. The magnetism,

on the other hand, is decreased while the north pole moves from d to b . The induction will be in the direction db while the magnet moves from a to c , and in the direction bd while it moves from c to a . The weakening and strengthening of the magnet, therefore, take place symmetrically with respect to the direction of the induction-shocks. While the induction-shock passes from b to d , the magnet is first strengthened and then weakened. While the induction-current passes from d to b , the magnet is first weakened and then strengthened. Such a difference in the magnetism could not, therefore, alter the strength of the induction-shocks; and they would counterbalance each other just as much as before, were it not for a secondary effect which, indeed, tends to produce the phenomenon that has to be explained. There will be an induction of currents in the coil due not to the motion of the magnet, but to its magnetization and demagnetization. While the north pole moves from b to c , the magnet is strengthened and the current in bd weakened



in consequence. While the north pole moves from *c* to *d*, the temporary magnetism dies away again, and thereby strengthens the current in *bd*. The consequence of this reaction of the magnetization on the current is that the maximum of magnetization does not take place when the north pole is at *c*, but only after it has passed *c*. Similarly the minimum does not take place when the north pole is at *a*, but only after it has passed *a*. The induction-shock is therefore stronger while the north pole of the rotating magnet moves from *c* to *a* than while it moves from *a* to *c*. The two induction-shocks are not of equal strength, but the direction will have the upper hand in which the permanent current goes. A phenomenon similar to the one observed would therefore take place. We have therefore to settle the question whether this effect would be sufficiently strong to produce any visible effect. This could best be done experimentally. While I was engaged with these experiments I did not think of this retardation of the maximum of magnetism, to which my attention has since been drawn by Professor Kirchhoff. I can therefore, unfortunately, offer no direct evidence on the subject; but I hope to be able to show that it is extremely unlikely, and even next to impossible, that the phenomenon is due to the cause just described. The strongest argument which I can offer regarding this point is this:—The magnetism induced by the permanent current must have been much weaker than the magnetism induced by the earth. The temporary magnetism induced by the earth did not, however, affect the experiments; and I conclude therefore that magnetism induced by the current did not affect the experiments.

That the magnetic force of the current was weaker than the magnetic force of the earth is shown as follows:—The tangent of the deflection of the galvanometer-needle never exceeded 0.2. This is therefore the ratio of the magnetic force of the current on the galvanometer-needle to the retaining force, which, as the needle was astatic, was much weaker than the earth's horizontal force. The magnetic force of the current on the galvanometer-needle must have been much stronger than that on the rotating magnet, as the resistance of the galvanometer was 2477 and that of the induction-coil only 30, the wires being about the same thickness. It follows that the magnetizing force of the current on the rotating magnet was much weaker than the magnetizing force of the earth. As the induction-coil was used in various positions, and no effect dependent upon that position was ever observed, I conclude that the effect of the earth's induction was too small to be observed, and consequently that the permanent current could not have produced any sensible alteration in the magnetism of the rotating magnet.

Many other observations agree with this conclusion. When, for instance, the galvanometer of small resistance was employed, a current about eighty times stronger passed through the induction-coil than when the usual galvanometer was used. It should be expected that if the effect were due to a magnetization of the magnet, it would be stronger in that case, if not eighty times, perhaps thirty or forty times. Yet the effect was hardly stronger than before, sometimes even not quite as strong. The numerical results obtained give additional evidence against the explanation which we have just discussed.

IV. *Two possible Explanations.*

As we cannot explain the effect by any known cause, we have to find another explanation. I can only account in two ways for the phenomenon which has to be explained; and my experiments give no clue as to which of them is the correct explanation. The first, and perhaps the simplest assumption to be made, is that Ohm's law is not rigidly correct. Ohm's law says that the resistance which a wire offers to an electric current going through it is independent of the strength of the current. I shall try to show now that my observations can be explained by assuming that the resistance decreases as the current increases, but that the deviation from Ohm's law would be so small that it would not have been found out by any of the usual methods of verifying Ohm's law. Suppose the electromotive force of the rotating magnet to be $+e$ in one direction and $-e$ in the other direction, and that the electromotive force sending the permanent current through the galvanometer-circuit is $+2x$. If the permanent current and the electrical vibrations go through the circuit at the same time, we have a current the electromotive force of which is $x+e$, counterbalancing another current the electromotive force of which is $x-e$. If the resistance for both cases is equal to r , we have as the resultant current affecting the galvanometer-needle

$$\frac{x+e}{r} + \frac{x-e}{r} = \frac{2x}{r}.$$

We should therefore have the same effect as if only the permanent current went through the circuit. If, however, the resistance of the first current is $r+dr$, we have for the whole effect

$$\frac{x+e}{r+dr} + \frac{x-e}{r}.$$

If dr is very small, this expression is equal to

$$\frac{2x}{r} - \frac{x+e}{r^2} dr.$$

If, therefore, dr be negative, the deflection will be greater when the electrical vibrations and the permanent current are passing than when the permanent current alone is passing. This means that the experiments described above could be explained by assuming that the resistance decreases as the current increases. As we can make e very large without introducing any difficulty in the way of experimenting, we may produce a sensible effect although dr may be very small. It can be shown that with the instruments with which I worked I could have detected a difference in the resistance amounting to $\frac{1}{100,000}$ of the whole resistance. The change in the resistance which we should have to assume in order to account for the effect observed is easily found. Supposing the current which passes when the constant electromotive $2x$ acts alone to be c , the deflection will then be proportional to c or αc . Similarly the resultant current which passes when the constant electromotive force acts and the magnet rotates shall be designated by c' , and the deflection observed consequently by $\alpha c'$. We have then

$$\alpha c = \frac{2x}{r},$$

$$\alpha c' = \frac{2x}{r} - \frac{x+e}{r^2} dr.$$

Dividing one of these equations by the other, we get

$$\frac{dr}{r^2} = \frac{c-c'}{c} \cdot \frac{2x}{x+e}.$$

In this equation x means the integral electromotive force of the battery acting during half a revolution of the magnet, while e means the electromotive force of the moving magnet during that time. The value of $\frac{2x}{x+e}$ in my experiments was generally about 0.001; $\frac{c'-c}{c}$ was found by experiments to be nearly 0.05. It follows from this that

$$\frac{dr}{r} = -0.00005.$$

A change in the resistance amounting to such a small fraction of the resistance only could not have been detected by the methods which have been hitherto used to verify Ohm's law.

I shall now pass to a second explanation of the phenomenon, which is, however, a little complicated. If a current of electricity passing through a circuit is increased by any electromotive force, an electromotive force in the opposite direction

is set up in the circuit, owing to what we call self-induction. It is assumed that this electromotive force is proportional to the rate of increase of the current; so that, if that rate is a negative quantity, an electromotive force in the same direction is set up. If we could imagine some state of things so that this self-induction would not depend merely upon the rate of increase of the current, but also in part upon the current passing through the circuit, my experiments would find an easy explanation. In order to fix our ideas I shall make certain hypotheses, and show how the different facts can be accounted for by means of them. While the current increases it has to establish its own lines of force, and is therefore doing work. The weakening of the current due to self-induction is assumed to be due to this work. The establishment of the lines of force may, however, not be the only work the current has to do. Suppose the current places the particles of copper in a certain way, and suppose that this magnetization of the copper particles, as it may be called, approaches a maximum as the current increases. It is easy to see that in this hypothetical case the self-induction of a current will be smaller if the original current passing through the circuit was strong than if it was weak. It is also evident that the self-induction will be weaker when a current is increased than when it is decreased by a certain amount. A rotating magnet will therefore induce currents of unequal strength in a wire through which a permanent current passes, as the current induced in the same direction as the original current will be stronger than that induced in the opposite direction. This would agree with the observed facts.

V. Conclusion.

It is impossible to decide by the experiments which I have made which of these two explanations is the correct one; and it will be very difficult to decide this by further experimentation. Whenever we use currents of varying intensity, both the causes suggested will give the same result. We should therefore have to use only constant currents. Unfortunately we have to meet great practical difficulties if we want to use constant currents. The greatest of these difficulties is the change of resistance by increase of temperature. I need not go into this any further here.

The above paper was read in a slightly different form before Section A of the British Association at Belfast. As it is of some importance to see whether Ohm's law is rigidly correct or not, the British Association has appointed a Committee for the purpose of verifying the law experimentally. If the result should be that the law is correct within the limits in which we should

expect a deviation from the experiments described in this paper, we should have only the second explanation to fall back upon.

The experiments were made in the physical laboratory of the University of Göttingen; and I have to thank Professor Weber and Professor Riecke for the kindness with which they have placed the necessary apparatus at my disposal.

XLVIII. *The Hydrodynamical Theory of the Action of a Galvanic Coil on an external small Magnet.*—Part II. By Professor CHALLIS, M.A., F.R.S.

[Continued from p. 200.]

THE discussions of principles and proofs of propositions, contained in Part I. in the September Number, have prepared the way for a direct solution on hydrodynamical principles of the problem of the action of a galvanic coil on a small magnetic needle, which I now proceed to give. For the sake of convenience in making references, the articles in this second Part are numbered in continuation of those of the first.

43. From the reasoning contained in arts. 24–38 it has been concluded that the motion of a galvanic current along a uniform conductor whose axis is a complete circle, and whose transverse section is a small circle of given radius, consists of circular motion parallel to the axis and of circular motion about the axis in transverse planes, the result being *spiral* motion, which, as shown in art. 42, may either be dextrorsum or sinistrorsum. A rectilinear axis being supposed to be drawn at right angles to the plane of the circular axis of the conductor through its centre, let R be the distance of any point of the fluid from that axis and r the distance of the same point from the axis of the conductor. Then it has been shown in arts. 35–38 that the transverse circular velocity is $\frac{c_1}{r^2}$, and the circular velocity parallel to

the conductor's axis is $\frac{c_2}{Rr}$, c_1 and c_2 being arbitrary constants indeterminately related to each other.

44. Now let a small magnet be so mounted that its oscillations about a centre of motion, supposed fixed, shall be constrained to take place in the plane which passes through that centre and the centre of the conductor's axis, and cuts the plane of this axis at right angles. From what is proved in arts. 17–19, to find the direction which the galvanic current gives to the axis of the small magnet we have only to find the direction, at the centre of its motion, of the velocity of the current [resolved in the plane of that motion; and it is permitted to consider separately the effects of the two motions which the current con-

sists of. But it is evident that the motion of the current parallel to the axis of the conductor has no effect in determining the angular position of the magnet, inasmuch as it cuts the plane in which the magnet is compelled to move at right angles. Also the small secondary motions generated by the reaction of the atoms against the portion of the current which flows in the interior of the conductor, if they should be of sensible amount, would have no tendency to cause vibrations of the magnet, because the aggregate directive effects of those on the opposite sides of the plane of vibration would be just equal and opposite. It remains only to consider the effect of the circular motions about the conductor in planes perpendicular to the plane of its axis.

45. Now these motions will have a directive effect on the magnet in two ways, which will require separate considerations. First, the angular position of the magnet will be determined in part, and in a *direct* manner, by the circular motions about the two opposite points of intersection of the axis of the conductor by the transverse plane passing through the centres of the magnet and of that axis. If A and B be these points and P be the given point, these motions, being in the same direction about the axis of the conductor, will be in *opposite* directions about the points A and B, and, according to the law stated in art. 43, will vary inversely as the squares of the distances AP and BP. Hence it is easy to calculate the sums of the resolved parts of the velocities in two rectangular directions, one parallel to the before-mentioned axis through the centre of the conductor, which will be supposed to be the axis of z , and the other parallel to the intersection of the plane of the circular axis by the plane through the axis of z and the point P. If we suppose the latter plane to be coincident with the plane zx , the resolved velocities will be parallel to the direction of the axes of z and x . Hence if z be the distance OC of the centre C of the circular axis from O the origin of coordinates, and if p and q be the coordinates of P parallel to the axes of z and x , and a be the radius of the circular axis, then, the two above-mentioned velocities being expressed by

$$\frac{\mu}{(p-z)^2 + (q-a)^2}, \text{ and } \frac{\mu}{(p-z)^2 + (q+a)^2},$$

and the sums of their resolved parts in the directions of the axes being X and Z, it will be found that

$$X = \frac{\mu(p-z)}{((p-z)^2 + (q-a)^2)^{\frac{3}{2}}} - \frac{\mu(p-z)}{((p-z)^2 + (q+a)^2)^{\frac{3}{2}}},$$

$$Z = - \frac{\mu(q-a)}{((p-z)^2 + (q-a)^2)^{\frac{3}{2}}} + \frac{\mu(q+a)}{((p-z)^2 + (q+a)^2)^{\frac{3}{2}}}.$$

It is here supposed that the direction of the motion about the axis is such that at the centre C, where the two motions have the same direction, the compound motion is in the *positive* direction; for since at that point $p=z$ and $q=0$, the formulæ give $X=0$, and $Z=+\frac{2\mu}{a^2}$.

46. The above values of X and Z determine the direction the needle would take supposing it to be acted upon solely by the two motions considered in the preceding argument. To find the effect of a coil composed of any number of such circular rheophores all of the same magnitude, and having the axis of z for their common axis, it is required to find $\int Xdz$ and $\int Zdz$, the integrals being taken from one end to the other of the axis of the coil. If the length of the axis be $2l$ and its middle point be at the origin of coordinates, the limits of the integration are $z=-l$ and $z=+l$. Hence substituting, for the sake of brevity,

n_1 for $((p-l)^2+(q-a)^2)^{-\frac{1}{2}}$, n_3 for $((p-l)^2+(q+a)^2)^{-\frac{1}{2}}$,

n_2 for $((p+l)^2+(q-a)^2)^{-\frac{1}{2}}$, n_4 for $((p+l)^2+(q+a)^2)^{-\frac{1}{2}}$,

it will be found by effecting the integrations that

$$k \int Xdz = k\mu(n_1 - n_2 - n_3 + n_4),$$

$$k \int Zdz = k\mu \left(\frac{p-l}{q-a} n_1 - \frac{p+l}{q-a} n_2 - \frac{p-l}{q+a} n_3 + \frac{p+l}{q+a} n_4 \right),$$

k being the constant factor which converts the sum of the *velocities* into the sum of the *directive forces*.

47. If instead of a single cylindrical coil we have any number of such coils in juxtaposition and having a common axis, the directive action of this compound coil would be found by obtaining the integrals

$$\int (k \int Xdz) da, \quad \int (k \int Zdz) da,$$

between the limits of the least and greatest values of the radius a . Supposing a_1 to be the mean value of a , and the least and greatest values to be $a_1 - \epsilon$ and $a_1 + \epsilon$, the results of these integrations, if ϵ be very small compared with a_1 , would be very approximately obtained by multiplying the previous integrals by 2ϵ , putting a_1 in the place of a , and altering the constant k .

48. The direct action of the transverse circular motions on the magnet having thus been calculated, we have now to take account of an *indirect* action, the hydrodynamical origin of which I proceed to explain. From hydrodynamics it is known that the steady motions of a mass of fluid of unlimited dimensions, whether it be incompressible or highly elastic, may be supposed to coexist, and that, if U, V, W be the sums of

the resolved parts of the velocities in three rectangular directions, the pressure p at any point is given by the equation

$$p = C - \frac{1}{2}(U^2 + V^2 + W^2).$$

In this equation U, V, W may represent the resolved parts of impressed velocities, such as the longitudinal and transverse velocities which, according to the present theory, constitute the galvanic current which is produced and maintained, by the action of a battery, along a fine cylindrical conductor. Hence, since the value of p , by reason of these velocities, varies from point to point of space, they give rise to motions of the fluid which, for distinction, may be called *secondary*; and the variations of pressure producing such motions, inasmuch as they result from *impressed* velocities, are to be regarded as externally impressed moving forces. The motions thus produced will be steady because the originating primary motions are steady; and consequently these secondary motions can coexist with the primary.

49. In the problem before us U, V, W may be taken to express the sums of the resolved parts of all the motions, as well those parallel to, as those transverse to, the axes of the conducting wires. But it is evident that the former may be left out of consideration, because they tend to produce equal and opposite motions on the opposite sides of the plane of the axis of each circular conductor, and therefore, on the whole, have no effect in generating secondary motions. Thus we have only to take account of the transverse motions about the circular axes.

50. Directing attention at first to a single circular rheophore and to the motions about its axis which cross the enclosed plane circular area, it will be seen that at each point of this area *two* motions about two elements of the axis, which are at the extremities of a diameter passing through the point, *have the same direction*. The sum of the velocities at or immediately contiguous

to the centre is $\frac{2\mu}{a^2}$, and is a minimum; it increases from the centre up to the conductor; but on the other side of the conductor in the extension of the same plane the two velocities are *opposed* to each other, and their sum rapidly decreases. Thus, the motion being steady, the mean of the motions crossing the circular area will be in excess, and the mean of the pressures in defect, within the area; and the contrary will be the case outside. The consequence will be that a current of the circumambient fluid will *set* through the area, entering at one side and issuing at the other, as being necessarily a circulating current. Each circle of the coil will have the same

effect; and as the different circles may be assumed to produce independent effects, there will be in the interior of the coil from end to end impulses of equal intensity analogous to those which were supposed in art. 7 to be generated in a magnet by a regular gradation of atomic density. The whole action being symmetrical with respect to the axis of the coil, the impulses may with close approximation be assumed, for the same reasons as in the case of the magnet, to be concentrated along the axis, supposing the shape of the coil to be that of a hollow cylinder, and the radii of the interior and exterior surfaces to be small.

51. In fact, so far as regards the indirect action now under consideration, the hydrodynamical circumstances due to the coil are exactly analogous to those due to a magnet, the force of the battery performing the part which was attributed to the gradation of atomic density of the magnet. Consequently the mathematical investigations contained in the Numbers of the Philosophical Magazine for July 1869 and June 1872, and already applied in the theory of the magnet given in arts 6-15 of the present essay, may with equal reason be applied to the problem of the action of a galvanic coil, although in each case the adopted solution of the hydrodynamical problem can only be considered to be approximately applicable. It suffices, however, for drawing the inference that the galvanic action of a cylindrical coil approximates to the action of a cylindrical magnet, which conclusion is confirmed by well-known experiments made with Ampère's *solenoids*. Accordingly the formulæ obtained in art. 15 for the directive action of a large magnet on a small magnetic needle will be considered to be equally applicable to the action of a galvanic coil on the needle, so far, at least, as regards the *indirect* action of the coil.

52. But experiment has also shown that there is a decided difference between the action of a magnet and that of a coil under the same circumstances. This fact has been specially established by the experiments of the Astronomer Royal the results of which are given in the Philosophical Transactions, vol. clxii. pp. 489-491. I propose to account for the difference by taking into consideration the *direct* effect of the circular motions treated of in arts. 45-47, to which there is nothing corresponding in the hydrodynamical circumstances of the streams of a magnet. This explanation admits of being supported by the following argument.

From results obtained in art. 15 it appears that if at any point P, the coordinates of which in the longitudinal and transverse directions are p and q , the directive force of a large magnet of length $2l$ on a small one in the longitudinal direction be Z_1 , and that in the transverse direction be X_1 ,

$$\frac{Z}{X_1} = \frac{\frac{p-l}{((p-l)^2 + q^2)^{\frac{3}{2}}} - \frac{p+l}{((p+l)^2 + q^2)^{\frac{3}{2}}}}{\frac{q}{((p-l)^2 + q^2)^{\frac{3}{2}}} - \frac{q}{((p+l)^2 + q^2)^{\frac{3}{2}}}},$$

the origin of the coordinates being the middle point of the axis. This ratio determines the direction of the magnetic current at the point P, and, according to our theory, gives also the direction of the current which would be produced at the same point if the magnet were replaced by a galvanic coil having an axis of the same length, so far, at least, as the current is due to what I have named the indirect action. If Z'_1 and X'_1 represent the directive forces in the same rectangular directions due to the direct action of the coil, it follows from the results obtained in arts. 46 and 47 that

$$\frac{Z'_1}{X'_1} = \frac{(p-l)\left(\frac{n_1}{q-a_1} - \frac{n_3}{q+a_1}\right) - (p+l)\left(\frac{n_2}{q-a_1} - \frac{n_4}{q+a_1}\right)}{n_1 - n_2 - n_3 + n_4}.$$

Substituting now for n_1, n_2, n_3, n_4 the expressions given in art. 46, after putting a_1 for a , and supposing that a_1 is small compared with the distance of the point P from the extremity of the axis, which distance is $((p-l)^2 + q^2)^{\frac{1}{2}}$, it will be found, by expanding the right-hand side of the above equation so as to include only the first power of a_1 , that

$$\frac{Z_1}{X_1} - \frac{Z'_1}{X'_1} = \frac{\left(1 + \left(\frac{q}{p+l}\right)^2\right)^{-\frac{1}{2}} - \left(1 + \left(\frac{q}{p-l}\right)^2\right)^{-\frac{1}{2}}}{\left(1 + \left(\frac{p-l}{q}\right)^2\right)^{-\frac{3}{2}} - \left(1 + \left(\frac{p+l}{q}\right)^2\right)^{-\frac{3}{2}}}.$$

Since, as it is easy to see, the right-hand side of this equality is a positive quantity, it follows that $\frac{Z_1}{X_1}$ is greater than $\frac{Z'_1}{X'_1}$. The meaning of this result is that the lines of motion

pertaining either to the current of a magnet or to the current indirectly produced by a galvanic coil, supposing the current to issue in the *positive* direction, are *less* bent from the axis than the lines of motion directly due to the circular motions about the axis of the coil. Hence it appears that the lines of motion due to the streams compounded of those resulting both from the indirect and the direct actions will be *more* bent from the axis than those of the magnet. These lines, therefore, supposing them to pass through all the points P of the circumference of a circle, the centre of which is on the axis of z and its plane per-

pendicular to that axis, will apparently diverge from a point of the axis *more* distant from the origin O than the point of apparent divergence of those lines of motion of the magnet which pass through the same points P. Analogous considerations, applied to the entering streams at the other end of the coil, would show that the points of apparent *convergence* of the lines of motion are more distant from O in the case of the coil than in that of the magnet. These inferences from the theory accord with the results of the experiments mentioned in art. 52.

53. According to what is said in art. 20, the velocities designated by X_1 , X'_1 , Z_1 , Z'_1 are proportional to the directive forces of the coil in the transverse and longitudinal directions. The theory has furnished an expression for $\frac{Z_1}{X_1}$, which ratio determines at any point the direction given to the axis of the small magnet, either by the streams of a large magnet or by those due to the indirect action of a coil; and it has also furnished the value of $\frac{Z'_1}{X'_1}$, which gives the direction that the axis of the small magnet would take if acted upon only by the transverse circular movements of the galvanic current. But we require to know the direction of the axis resulting from the total action of the coil, the expression for determining which, on the principle of the coexistence of velocities, will be

$$\frac{Z_1 + Z'_1}{X_1 + X'_1}.$$

It is here to be remarked that, according to the conditions of the problem, there must be a certain hydrodynamical relation between Z_1 and Z'_1 , as also between X_1 and X'_1 ; but in the existing state of hydrodynamics it does not appear possible to ascertain these relations exclusively by theoretical investigation. Hence for calculating the above ratio recourse must be had to experimental data. Theory has advanced so far as to prove that $Z_1 = C \times M$, $X_1 = C \times M'$, $Z'_1 = C' \times N$, $X'_1 = C' \times N'$, M , M' , N , N' being quantities that can be calculated from known formulæ, and C , C' being unknown constants. If, now, for a certain position of the centre of the small needle the value of the ratio which determines the angular position of its axis be found by experiment to be β , we shall have

$$\beta = \frac{Z_1 + Z'_1}{X_1 + X'_1} = \frac{C_1 M + C'_1 N}{C M' + C' N'} = \frac{M + C_1 N}{M' + C'_1 N'}$$

C_1 being put for $\frac{C'}{C}$. Consequently $C_1 = \frac{M - M'\beta}{N'\beta - N}$. By taking

different positions we may calculate from this formula as many different values of C_1 as we please; and it will be a criterion of the truth of the theory to find that these values differ but little from each other, C_1 being, according to the theory, absolutely constant. Taking the mean of the values of C_1 thus obtained to be a sufficiently close approximation to its true value, we may proceed to calculate for any given position of the centre of the small needle the angular position of its axis resulting from the total action of the coil, by means of the formula

$$\frac{Z_1 + Z'_1}{X_1 + X'_1} = \frac{M + C_1 N}{M' + C_1 N'}$$

M, N, M', N' being calculated for the given position by means of the known formulæ. By comparisons of angular positions of the needle calculated in this manner with a large number of observed positions, the theory may be fully tested.

54. Since I arrived at this solution of the problem, I have not had leisure for going through the arithmetical calculation which would be required for making the above-mentioned comparisons, and I am consequently unable to say whether the theory is capable of satisfying such a test. This omission, however, does not materially affect the argument, because, as I shall afterwards show, the theory may be tested in another way. I have already intimated in arts. 5 and 24 that on the principles of the method of philosophy I have adopted it should be possible to account theoretically for the facts and hypotheses on which Ampère's galvanic theory of magnetism is founded. If this were effected by means of the hydrodynamical theory of galvanism, it is evident that any confirmation which Ampère's theory might receive by satisfying an applied test, would at the same time be a confirmation of the hydrodynamical theory. For instance, the computations undertaken by Mr. Stuart for the purpose of accounting for, on Ampère's theory, the results of the Astronomer Royal's experiments on the Directive Power of Galvanic Coils in their action on external small magnets (given in the memoir cited in art. 52), and the comparisons contained in an Appendix to the memoir, might, in the case supposed, be claimed for the hydrodynamical theory. Accordingly I propose to give in a third part a theoretical discussion of the empirical foundations of Ampère's theory, with particular reference to its application in the problem of the galvanic coil.

Cambridge, September 29, 1874.

Postscript, Oct. 7, 1874.—In the last paragraph of the fore-

going communication I have stated that I omitted, from want of leisure, to compare the theory with observation by arithmetical calculations. Part II. was, in fact, finally written out for the press before I took any steps for making such comparison, being deterred from the undertaking mainly by the apprehension that it would require more time and labour than I could spare for it. I have, however, since found that the theory could be sufficiently tested by an amount of calculation much less than that which I thought to be necessary; and I now propose to give the results of such calculation.

According to the proposed hydrodynamical theory the action of a cylindrical coil on an external small magnet consists of two parts, which I have named "direct" and "indirect." The formulæ for calculating the angle of position of the magnet, supposing it to depend only on the indirect action, are given in art. 15; and those for calculating the position as depending only on the direct action are given in arts. 46 and 47. The theory is incapable of furnishing of itself formulæ for calculating the effect of the simultaneous action of the two parts; but it is shown in art. 53 how the calculation may be performed by the aid of experimental data.

The first set of formulæ were derived from the hydrodynamical theory of *magnetic* force, supposing the form of the magnet to be cylindrical, its transverse section small, and its ends to be *flat*, on the principle that under these conditions the magnetic action may be supposed to be concentrated in an elementary *line* of uniform magnetic intensity along the axis (see arts. 12–14). Then, for the reasons given in arts. 50 and 51, the same supposition is made relative to the galvanic coil; that is, as far as regards the indirect action, it is supposed to act exactly in the same manner as a magnet of the same length, having the form described above, provided also its transverse dimensions be small compared with its length. This last condition was satisfied in the experiments about to be used for testing the theory, the interior radius of the coil being 0·45 inch, the exterior radius 0·7 inch, and its length 13·4 inches. In calculating thus the indirect action the exact transverse dimensions do not come into account.

It is evident that the action will be the same in all planes passing through the axis of the magnet, or coil; and since the motion of the æther in any such plane is symmetrical both with respect to this axis and the transverse plane through its middle point, it will suffice to consider the lines of motion of the stream in only one of the quadrants. According to our theory the stream of æther issues from that end of a magnetic needle which is directed *southward*, which, as represented in the diagram in the Philosophical Transactions, vol. clxii. p. 488, is on the *right*

hand. The courses selected for consideration are those indicated by the small magnets on the upper side of the right-hand half of the diagram; and the longitudinal velocity will be taken to be positive in the direction of *issuing*, and the transverse velocity positive in the direction *from* the axis. Hence, putting μX_1 and μY_1 for these velocities respectively, and substituting m_1 for $((p-l)^2 + q^2)^{-\frac{3}{2}}$, and m_2 for $((p+l)^2 + q^2)^{-\frac{3}{2}}$, we have by the formulæ in art. 15,

$$-\frac{Y_1}{X_1} = \frac{q(m_1 - m_2)}{(p+l)m_2 - (p-l)m_1},$$

$2l$ being the length of the coil, and p, q the longitudinal and transverse coordinates of any one of the positions of the centre of the small magnet. If a line drawn in the direction *opposite* to that of the motion at any position cuts the axis of the coil at an angle of inclination ω reckoned always from the *negative* direction from the point of intersection, we shall have $\tan \omega = -\frac{Y_1}{X_1}$, and ω will in all cases be positive, varying from zero for a position on the transverse axis to 180° for a position on the longitudinal axis.

The calculation of ω by means of the formula above gives the theoretical determination of the angle of position of the small magnet, so far as the action of the coil is the same as that of a magnet; and the observed position is readily determined by means of the values of the longitudinal and transversal forces for a coil without core, given (in p. 491) in the memoir before cited. I have besides calculated the same angle by Ampère's theory, availing myself for that purpose of the results of Mr. Stuart's theoretical calculation of X and Y contained in p. 497. The value of l for all the observations is 6.7 inches, and the values of the coordinates p and q for the different positions of the small magnet are those subjoined. Both the experimental and the theoretical values of ω were calculated to minutes of arc, but it was thought sufficiently accurate, considering the circumstances of the observations, to express them to the nearest tenth of a degree. After these explanations the following statement of the results of the calculations will be intelligible.

No. of the series.	p .	q .	Value of ω by observation.	Value of ω by the theory.	Excess of observed value.	Excess by Ampère's theory.
	in.	in.				
1.	0.00	2.26	0.0	0.0	0.0	0.0
2.	1.34	2.26	9.0	10.5	-1.5	-1.1
3.	2.68	2.26	20.9	21.8	-0.9	-0.7
4.	4.02	2.26	35.8	35.8	0.0	+1.1
5.	5.36	2.26	57.0	56.9	+0.1	+2.2
6.	6.70	2.26	86.9	88.4	-1.5	-0.6
7.	7.78	1.70	121.8	121.6	+0.2	(-3.4)
8.	8.20	0.74	154.8	153.5	+1.3	-0.6
9.	0.00	3.73	0.0	0.0	0.0	0.0
10.	1.34	3.73	12.2	14.6	-2.4	-1.8
11.	2.68	3.73	27.5	29.6	-2.1	-1.5
12.	4.02	3.73	44.3	45.8	-1.5	-0.3
13.	5.36	3.73	72.2	64.6	(+7.6)	(+6.4)
14.	6.70	3.73	82.4	86.0	-3.6	-3.8
15.	7.83	3.44	102.9	105.2	-2.3	-1.7
16.	8.82	2.80	122.8	125.1	-2.3	+0.3
17.	9.49	1.82	144.6	145.8	-1.2	-0.4
18.	9.70	0.73	164.4	165.9	-1.5	-0.8

Respecting these results, it is first to be noticed that the observed and calculated values of ω differ so little from each other (except in the instances of Nos. 7 and 13, which seem to be affected by some incidental errors), that the differences scarcely exceed what might be attributed to unavoidable errors of observation. It is evident, therefore, that the effect of not taking the direct action into account must be very small; and I have consequently not thought it worth while to employ the calculations indicated in art. 53 for ascertaining its amount. The reason that the direct action has comparatively so little effect I take to be, its being due only to the transverse circular motions about parts of the coil contiguous to the plane passing through the axis of the coil and the centre of the small magnet in a *very thin slice*; whereas the indirect effect results from the transverse motions about all parts of the coil, and increases both with its length and with the *area* of its transverse section.

It may, however, be remarked that there is a preponderance of *minus* signs in the excesses of the observed values, and that this is the case in somewhat greater degree relatively to the hydrodynamical theory than to that of Ampère. The argument in art. 52 shows that, by taking account of the direct action, the angle ω would be *diminished*, so that the *minus* errors might thus be made less, and the calculated and observed values be brought into closer agreement. In the calculation of the *direct* action the transverse dimensions of the coil are explicitly taken

into account (see art. 52); and in Ampère's theory the same thing is done by making the hypothesis of a magnetic *sheet*. This probably explains why that theory agrees with experiment in some degree better than the hydrodynamical theory when the latter does not include the direct action.

The hydrodynamical theory admits also of the application of a different arithmetical test, based on the following considerations. The theoretical quantities μX_1 and μY_1 are *velocities of the æther*; and supposing X' and Y' to be the directive *forces* acting on the small magnet, expressed numerically by inference

from the experiments, we have seen that *quam proximè* $\frac{Y_1}{X_1} = \frac{Y'}{X'}$.

This equality would be generally true if the ratio of Y_1 to Y' were the same as that of X_1 to X' , even if that ratio were different for different positions of the magnet. But the argument in art. 19 shows that the directive forces in the two rectangular directions are equal to the quantities X_1 and Y_1 multiplied by a *constant factor*, which, although dependent on the dimensions and atomic constitution of the small magnet, is independent of its position and the direction of its axis. Consequently for each set of corresponding values we have $Y' = kY_1$ and $X' = kX_1$, k being absolutely constant; and by summing all these equalities,

$$\Sigma . Y' + \Sigma . X' = k(\Sigma . Y_1 + \Sigma . X_1),$$

which equation may be supposed to give the value of k with as much accuracy as the character of the experiments allows of. By calculating X_1 and Y_1 from the expressions within brackets in the formulæ of art. 15, taking X' and Y' as given numerically by the experiments, and using *all* the values of X_1, Y_1, X', Y', I find that the value of k which satisfies the above equality is 6973·7.

Assuming that in Ampère's theory $X' = k'X_2$, $Y' = k'Y_2$, and that the values of X_2, Y_2 are those obtained for X and Y by Mr. Stuart's calculation, I have found, by a process exactly analogous to that indicated above, that $k' = 1\cdot6373$.

The values of the constant factors k and k' being thus determined, we may proceed to test both theories by comparing the several values of kX_1 and kY_1 , and those of $k'X_2$ and $k'Y_2$, with the corresponding values of X' and Y' . In this way the following results have been obtained:—

No. of the series.	X'	$X' - kX_1$	$X' - k'X_2$	Y'	$Y' - kY_1$	$Y' - k'Y_2$
1.	-216	+48	+ 46	0	0	0
2.	-240	+46	+ 35	38	- 15	-11
3.	-315	+44	+ 26	120	- 23	-14
4.	-450	+41	+ 36	325	- 29	-12
5.	-550	+11	+ 30	848	- 12	+24
6.	- 89	-43	- 18	1480	+121	+80
7.	+1010	+121	+121	1630	+183	(+368)
8.	+2480	+276	+160	1170	+ 69	+44
9.	-184	+23	+ 19	0	0	0
10.	-189	+26	+ 21	41	- 15	-11
11.	-200	+34	+ 28	104	- 28	-22
12.	-217	+27	+ 29	212	- 39	-30
13.	-123	(+69)	(+ 55)	383	- 22	-15
14.	- 57	-22	- 24	424	- 68	-59
15.	+100	-36	- 31	436	- 62	-68
16.	+264	-50	- 29	410	- 36	-50
17.	+475	-25	- 46	338	- 2	-27
18.	+668	-17	- 79	186	+ 14	-10

The anomalies here presented by Nos. 7 and 13, taken in connexion with those in the former comparison, seem to point to errors concerned with the computing of Y_2 and observing of X' . The differences $X' - kX_1$ and $Y' - kY_1$ between the observed and calculated forces according to the hydrodynamical theory, and the differences $X' - k'X_2$ and $Y' - k'Y_2$ according to Ampère's theory, are perhaps not greater, when compared with the values of X' and Y' , than might from the circumstances of the observations be expected. Relative to these differences it may be remarked, as before, that the hydrodynamical theory, restricted to the indirect action, does not agree so well with observation as the theory of Ampère. The totality, however, of the above comparisons with the former theory prove that the value of k must at least be very approximately constant, and so far justify the theoretical conclusion that for the case of a coil, or a magnet, reduced to a *line* of magnetism, that factor is absolutely constant.

But at the same time the foregoing comparisons with Ampère's theory prove that the factor k' is also very nearly constant, although there is no *à priori* reason deducible from that theory why this should be the case. It is worthy of remark, too, that the differences between observation and calculation exhibited above follow for the most part the same law for both theories, although they depend on very different processes of calculation. I cannot but regard these circumstances as confirmatory of the views maintained in the foregoing essay, according

to which the received empirical theories of galvanism and magnetism virtually have for their basis the hydrodynamical theory of these forces.

I have not attempted to treat in like manner the other experiments the details of which are given in the same memoir, because my analysis applies exclusively to a cylindrical magnet, or coil, of the same transverse section throughout. The action of a large bar-magnet, and that of an iron core in the form of a *bar* within a coil, would require very different treatment, on account of the forms not being symmetrical with respect to an axis. The experiments made with the edge and flat side of the large bar-magnet turned towards the small magnet showed that the action depended in some degree on the *form* presented to the latter. I take occasion to mention incidentally that the great accession of magnetic force which was observed to result from putting a soft-iron core in the coil is attributable, according to the hydrodynamical theory, to the transverse circular motions about the rheophore constituting the coil, which give rise to just such ætherial streams as are proper for magnetizing. (See ‘Principles of Physics,’ pp. 619 & 620.)

The arguments, and the evidence from numerical comparisons, which I have now adduced, should, I think, suffice for coming to the conclusion that galvanic and magnetic forces are modes of pressure of the æther in steady motion.

XLIX. *On the Perturbations of the Compass produced by the rolling of the Ship.* By Sir WILLIAM THOMSON, F.R.S.*

THE “heeling-error,” which has been investigated by Airy and Archibald Smith, is the deviation of the compass produced by a “steady heel” (as a constant inclination of the ship round a longitudinal axis, approximately horizontal, is called). It depends on a horizontal component of the ship’s magnetic force, introduced by the inclination; which, compounded with the horizontal component existing when the ship is upright, gives the altered horizontal component when the ship is inclined. Regarding only the error of direction, and disregarding the change of the intensity of the directing force, we may define the heeling-error as the angle between the directions, for the ship upright and for the ship inclined, of the resultant of the horizontal magnetic forces of earth and ship at the position of the compass. These suppositions would be rigorously realized with the compass supported on a point in the ordinary manner, if the

* Communicated by the Author, having been read in Section A. of the British Association at Belfast (1874).

bearing-point were carried by the ship uniformly in a straight line. They are nearly enough realized in a large ship to render inconsiderable the errors due to want of perfect uniformity of the motion of the bearing-point, if this point is placed anywhere in the "axis of rolling"*; for in a large ship the compass, however placed, is not considerably disturbed by pitching, or by the inequalities of the longitudinal translatory motion caused by waves. Hence, supposing the compass placed in the axis of rolling, the perturbation produced in it by the rolling will be solely that due to the variation of the horizontal component of the ship's magnetic force. Such a position of the compass would have one great advantage—that the application of proper magnetic correctors adjusted by trial to do away with the rolling-error, would perfectly correct the heeling-error. To set off against this advantage there are two practical disadvantages:—one, that the axis of rolling (being always below deck) would not be a convenient position for the ordinary modes of using the compass; the other (far more serious), that, at all events in ships with iron decks, the magnetic disturbance produced by the iron of the ship would probably be so much greater at any point of the axis of rolling, than at suitably chosen positions above deck, as to more than counterbalance the grand kinetic advantage of the axial position. But careful trials in ships of various classes ought to be made; and it *may* be found that in some cases the compass may, with preponderating advantage, be placed at the axis of rolling. Hitherto, however, this position for the compass has not been used in ships of any class, and, as we have seen, it is not probable that it can ever be generally adopted for ships of all classes. It is therefore an interesting and important practical problem to determine the perturbations of the compass produced by oscillations or other non-uniform motions of the bearing-point.

The general kinetic problem of the compass is to determine the position at any instant of a rigid body consisting of the needles, framework, and fly-card, which for brevity will be called simply *the compass*, movable on a bearing-point, when this point moves with any given motion. Let the bearing-point experience at any instant a given acceleration α , in any given direction. Let W be the mass (or weight) of the compass, and gW the force of gravity upon it, reckoned in kinetic units. The position of kinetic equilibrium of the compass at that instant is the posi-

* One way, probably the best in practice, of finding by observation the position of the axis of rolling is to hang pendulums from points at different levels in the plane through the keel perpendicular to the deck, till one is found which indicates the same degrees of rolling as those found geometrically by observing a graduated scale (or "batten") seen against the horizon.

tion in which it would rest under the magnetic forces and a force of *apparent gravity* equal to the resultant of gW and a force αW in the direction opposite to that of α . Now the weight of the compass is so great and its centre of gravity so low that the level of the card is scarcely affected sensibly by the greatest magnetic couple experienced by the needles*. Hence in kinetic equilibrium the plane of the compass-card is sensibly perpendicular to the direction of the "apparent gravity" defined above; and the magnetic axis of the needles is in the direction of the resultant of the components, in this plane, of the magnetic forces of earth and ship. Hence it is simply through the *apparent level*, at the place in the ship occupied by the compass, differing from the true gravitation-level, that the problem of the kinetic-equilibrium position of the compass in a rolling ship differs from the problem of the heeling-error referred to above. That we may see the essential peculiarities of our present problem, let there be no magnetic force of the ship herself or cargo. The kinetic-equilibrium position of the magnetic axis of the compass will be simply the line of the component of terrestrial magnetic force in the plane of the apparent level. Let κ be the inclination of this plane to that of the true gravitation-level, and ϕ the azimuth (not greater than 90°) from magnetic north of the line LL' of the intersection of the two planes (a diagram is unnecessary); also let H and Z be the horizontal and vertical components of the terrestrial magnetic force. The component of this force in the plane of apparent level will be the resultant of $H \cos \phi$ along LL' and $H \sin \phi \cos \kappa + Z \sin \kappa$ perpendicular to LL' ; and therefore, if ϕ_1 denote the angle at which it is inclined to LL' , we have

$$\tan \phi_1 = \frac{H \sin \phi \cos \kappa + Z \sin \kappa}{H \cos \phi} = \tan \phi \cos \kappa + \frac{Z \sin \kappa}{H \cos \phi}.$$

If, as usual in compass questions, we reckon the *directions* as of forces on *south* magnetic poles (or the northern ends of the compass-needles), the direction of $H \cos \phi$ is along LL' *northwards*, and the direction of $Z \sin \kappa$, when the ship is anywhere north of the magnetic equator, is *downwards* in the plane of the apparent level.

Now, as we are only considering the effect of rolling, the direction of the given "acceleration" of the bearing-point will always be in a plane perpendicular to the ship's length; and therefore LL' will be parallel to the length. (It will in fact be the line through the "lubber-points" of the compass-bowl.) Hence, and as compass angles are ordinarily read in the plane of the fly-card, the kinetic equilibrium-error of the compass is

* Generally no adjusting counterpoise for the compass is required when a ship goes from extreme north to extreme south magnetic latitudes.

exactly equal to $\phi_1 - \phi$. When κ is a small fraction of $57^\circ.3$ (the "radian," as the angle whose arc is equal to radius has been called by Professor James Thomson), which is the case except in extreme degrees of rolling when the compass is properly placed*, we have approximately

$$\phi_1 - \phi = \kappa \frac{Z}{H} \cos \phi.$$

The direction of this error is, for the northern ends of the needles in the northern magnetic hemisphere or for the southern ends in the southern hemisphere, *towards the side on which the apparent level is depressed*—that is (as practically the compass is always *above* the axis of rolling), *towards the elevated side of the ship*. It has its maximum value

$$\kappa \frac{Z}{H}$$

when $\phi = 0$; that is to say, when the ship heads north or south magnetic. To estimate its amount, consider perfectly regular rolling; which in general fulfils approximately the simple harmonic law, so that we may put

$$i = I \sin nt,$$

where i denotes the inclination of the ship at time t , and n and I constants. Let h denote the height of the bearing-point of the compass, vertically above the axis of rolling when the ship is vertical. For the amount of its acceleration we have

$$\alpha = \frac{d^2(hi)}{dt^2} = -n^2 hi.$$

Now, if l denote the length of a simple pendulum isochronous with the rolling of the ship, we have

$$n^2 = \frac{g}{l},$$

and therefore

$$\alpha = -g \frac{h}{l} i.$$

The direction of α , being tangential to the circle described by the bearing-point, is approximately horizontal; and therefore the direction of apparent gravity will be approximately that of the resultant of

g vertical,

* The "mast-head compass," perniciously used in too many merchant steamers, may, in moderate enough rolling, experience deviations of apparent level amounting to 20° or 30° on each side of the true gravitation-level.

and

$$g \frac{h}{l} i \text{ horizontal.}$$

Hence

$$\kappa = \frac{h}{l} i, \text{ approximately.}$$

Hence, when the ship heads north or south, the amount of the kinetic-equilibrium error is approximately

$$\frac{Z}{H} \frac{h}{l} i.$$

Suppose, for example, the period of the rolling to be 6 seconds* (or three times the period of the "seconds' pendulum"); l will be 29 feet (or nine times the length of the seconds' pendulum). And suppose the compass to be $14\frac{1}{2}$ feet above the axis of rolling. We have $\kappa = \frac{1}{2} i$ (so that the range of apparent rolling indicated by a pendulum hung from a point in the position of the bearing-point of the compass is greater by half than the true range of the roll). On these suppositions the kinetic-equilibrium error amounts to

$$\frac{1}{2} \frac{Z}{H} i.$$

About the middle of the British Islands the magnetic dip is 70° , and therefore $\frac{Z}{H}$ (being the natural tangent of the dip) is equal to 2.75 nearly. Hence the kinetic-equilibrium error for the supposed case amounts in this locality to about a degree and three eighths for every degree of roll.

In an iron ship the equilibrium value of the rolling-error will be approximately the sum of the kinetic error investigated above, and a heeling-error found by an investigation readily worked out from that of Archibald Smith in the 'Admiralty Compass Manual' (edit. 1869, Section IV. pages 82-89, and Appendix, pages 139-150), with modification to take into account the deviation of the apparent level, at the place of the compass, from the true gravitation-level.

I have used the expression "kinetic-equilibrium error" to distinguish the error investigated above from that actually exhibited by the compass. It is exactly the error which would be shown by an ideal compass with infinitely short period of vibration. A light quick needle (either with silk-fibre suspension, or

* This would be the case for a ship of any size exposed to regular waves of length 184 feet from crest to crest, and, if moving through the water, moving in a line parallel to the lines of crests.

supported on a point in the ordinary way) having a period of not more than about two seconds, shows the rolling-error very beautifully, taking at every instant almost exactly the position of kinetic equilibrium. I have thus found the rolling and pitching errors so great in a small wooden sailing-vessel that it became very difficult to make exact observations with the quick compass, either in the Frith of Clyde or out at sea on the Atlantic unless when the sea was exceptionally smooth. The well-known kinetic theory of "forced oscillations" is readily applied to calculate, whether for a wooden or an iron ship, the actual "rolling-error" of the compass, from the "kinetic-equilibrium error" investigated above. Thus let

- u be the deviation of the compass at any instant, from the position it would have if the ship were at rest and upright ;
- T the period of its natural oscillation if unresisted by any "viscous" influence (the *damping* effect of copper, introduced by Snow Harris and used with good effect in the Admiralty standard compass, being included in this category) ;
- $2f$ a coefficient measuring the amount of viscous resistance ;
- E the extreme equilibrium value of the rolling-error ;
- T' the period of the rolling.

For brevity put $n = \frac{2\pi}{T}$, and $n' = \frac{2\pi}{T'}$. The differential equation of the motion is

$$\frac{d^2u}{dt^2} + 2f \frac{du}{dt} + n^2u = n^2 E \cos n't.$$

The integral of this proper to express the effect of regular rolling is

$$u = -E \frac{n^2 \cos (n't + \epsilon)}{\sqrt{\{(n'^2 - n^2)^2 + 4n'^2 f^2\}}},$$

where

$$\epsilon = \tan^{-1} \frac{2n'f}{n'^2 - n^2}.$$

It would extend the present communication too far to enter on details of this solution. For the present it is enough to say that no admissible degree of viscous resistance can make the rolling-error small enough for practical convenience, unless also the period of the compass is longer than that of any considerable rolling to which the ship may be subjected. Probably a period of from 15 to 30 seconds (such as an ordinary compass has) may be found necessary for general use at sea ; and it becomes an important practical question how is this best to be

obtained consistently with the smallness of the compass-needles necessary for a thoroughly satisfactory application of the system of magnetic correctors by which Airy proposed to cause the compass in an iron ship to point correct magnetic courses on all points?

L. *On the Spectrum of Carbon.* By W. MARSHALL WATTS, D.Sc., *Physical-Science Master in the Giggleswick School*.*

ALTHOUGH the different comets which have appeared in the northern skies since astronomers have been in possession of the spectroscope have been carefully examined by some of the best observers, the exact nature of the comet-spectrum is still a matter of doubt; and the very important question whether comets give the spectrum obtainable from carbon-compounds, or only a spectrum of bands of nearly the same refrangibility, is not decided.

The explanation of the varying results of different observers is to be found, no doubt, in the extreme faintness of the light emitted by a comet, and the great difficulty of measuring the positions of the lines by any arrangement which requires the bands to be seen together with cross-wires or spider-lines. The best chance of obtaining accurate results is probably to abandon micrometric measurements, and to work by eye-estimations of the distance of the bands from the known bands of some equally faint spectrum, made to occupy the lower portion of the field of view, provided a faint spectrum can be found *possessing a sufficient number of well-defined bands in the region of the spectrum to be mapped.*

In the case of the comet-spectrum we have just the reference-spectrum required in the well-known spectrum of carbon, which, if it be not identical with the comet-spectrum, has at all events bands of very nearly the same refrangibility, and can easily be obtained of any feeble intensity required.

It seemed to me therefore of importance to determine the positions of the lines of the carbon-spectrum with as much accuracy as the spectroscopic means at my disposal allow.

The spectrum was obtained from the flame of olefiant gas and oxygen, burnt together at the platinum nozzle of an oxyhydrogen blowpipe.

The spectroscope employed was Browning's automatic spectroscope of six prisms, with a micrometer eyepiece furnished with two pairs of cross-wires. This eyepiece requires 12·49 turns of the micrometer-screw to separate the wires by the in-

* Communicated by the Author.

terval between the lithium- orange line (6101) and the least-refrangible sodium-line (5895).

The reference-lines employed were, as far as available, the Fraunhofer lines of the solar spectrum, and the lines of the spark-spectra of magnesium, lead, air, antimony, cadmium, and zinc.

The results are given in the following Table. Column I. gives the designations of the lines measured as given in my 'Index of Spectra;' column II. the wave-lengths as now determined in tenth-metres; column III. the number of observations; column IV. the difference of the highest and lowest results from the mean result.

I.	II.	III.	IV.
$\gamma \left\{ \begin{array}{l} 58\cdot0 \\ 60\cdot0 \\ 61\cdot5 \\ 63\cdot0 \\ 64\cdot5 \end{array} \right.$	$\begin{array}{l} 5634\cdot7 \\ 5585\cdot5 \\ 5542\cdot3 \\ 5503\cdot5 \\ 5478\cdot4 \end{array}$	$\begin{array}{l} 11 \\ 7 \\ 2 \\ 1 \\ 1 \end{array}$	$\begin{array}{l} +1\cdot3, -1\cdot4 \\ +0\cdot5, -0\cdot6 \\ +0\cdot0, -0\cdot0 \end{array}$
$\delta \left\{ \begin{array}{l} 75\cdot0 \\ 77\cdot0 \\ 79\cdot3 \end{array} \right.$	$\begin{array}{l} 5165\cdot5 \\ 5130\cdot4 \\ 5100\cdot0 \end{array}$	$\begin{array}{l} 4 \\ 7 \\ 2 \end{array}$	$\begin{array}{l} +0\cdot3, -0\cdot5 \\ +1\cdot0, -2\cdot1 \\ +0\cdot8, -0\cdot8 \end{array}$
$\epsilon \left\{ \begin{array}{l} 97\cdot0 \\ 98\cdot5 \\ 100\cdot0 \\ 101\cdot5 \\ 101\cdot7 \end{array} \right.$	$\begin{array}{l} 4739\cdot8 \\ 4717\cdot2 \\ 4698\cdot4 \\ 4684\cdot2 \\ 4677 \end{array}$	$\begin{array}{l} 4 \\ 5 \\ 4 \\ 2 \\ 1 \end{array}$	$\begin{array}{l} +2\cdot2, -3\cdot6 \\ +2\cdot1, -3\cdot3 \\ +1\cdot5, -3\cdot2 \\ +1\cdot8, -1\cdot8 \end{array}$

The lines most exactly determined are those whose wave-lengths are 5165·5 and 5585·5.

It may be well to repeat here that this spectrum is the spectrum of carbon, and not of a hydrocarbon or any other compound of carbon. That it is so is proved by the fact that it is common to compounds of carbon with oxygen, with hydrogen, and with nitrogen (Phil. Mag. S. 4. vol. xxxviii. p. 249); and this evidence does not rest only upon the use of vacuum-tubes, the results obtained from which are confessedly open to doubt. The spectrum is not only obtained from the flame of olefiant gas and cyanogen or oxygen, but also from the electric spark taken directly in a stream of cyanogen (or carbonic oxide) at the ordinary pressure.

LI. *Researches in Acoustics.*—No. V.

By ALFRED M. MAYER.

[Continued from p. 274.]

3. *Experiments on the supposed Auditory Apparatus of the Culex mosquito.*

OHM states in his proposition that the ear experiences a simple sound only when it receives a pendulum-vibration, and that it decomposes any other periodic motion of the air into a series of pendulum-vibrations, to each of which corresponds the sensation of a simple sound. Helmholtz, fully persuaded of the truth of this proposition, and seeing its intimate connexion with the theorem of Fourier, reasoned that there must be a cause for it in the very dynamic constitution of the ear; and the previous discovery by the Marquis of Corti of several thousand* rods of graded sizes in the ductus cochlearis, indicated to Helmholtz that these were suitable bodies to effect the decomposition of a composite sonorous wave by their covibrating with its simple harmonic elements. This supposed function of the Corti organ gave a rational explanation of the theorem of Ohm, and furnished “a leading-thread” which conducted Helmholtz to the discoveries contained in his renowned work *Die Lehre von den Tonempfindungen*†. In this book he first gave the true explanation of timbre, and revealed the hidden cause of musical harmony, which, since the days of Pythagoras, had remained a mystery to musicians and a problem to philosophers.

It may perhaps never be possible to bring Helmholtz’s hypothesis of the mode of audition in the higher vertebrates to the test of direct observation, from the apparent hopelessness of ever being able to experiment on the functions of the parts of the inner ear of mammalia. The cochlea, tunnelled in the hard temporal bone, is necessarily difficult to dissect; and even when

* According to Waldeyer, there are 6500 inner and 4500 outer pillars in the organ of Corti.

† “But all of the propositions on which we have based the theory of consonance and dissonance rest solely on a minute analysis of the sensations of the ear. This analysis could have been made by any cultivated ear without the aid of theory; but the leading-thread of theory and the employment of appropriate means of observation have facilitated it in an extraordinary degree.

“Above all things I beg the reader to remark that the hypothesis on the covibration of the organs of Corti has no immediate relation with the explanation of consonance and dissonance, which rests solely on the facts of observation, on the beats of harmonics and of resultant sounds.”—Helmholtz, *Tonempfindungen*, p. 342.

a view is obtained of the organ of Corti, its parts are rarely *in situ*, and often they have already had their natural structure altered by the acid with which the bone has been saturated to render it soft enough for dissection and for the cutting of sections for the microscope.

As we descend in the scale of development from the higher vertebrates, we observe the parts of the outer and middle ear disappearing, while at the same time we see the inner ear gradually advancing toward the surface of the head. The external ear, the auditory canal, the tympanic membrane, and with the latter the now useless ossicles, have disappeared in the lower vertebrates, and there remains but a rudimentary labyrinth.

Although the homological connexions existing between the vertebrates and articulates, even when advocated by naturalists, are certainly admitted to be imperfect, yet we can hardly suppose that the organs of hearing in the articulates will remain stationary or retrograde, but rather that the essential parts of their apparatus of audition, and especially that part which receives the aërial vibrations, will be more exposed than in higher organisms. Indeed the very minuteness of the greater part of the articulates would indicate this; for a tympanic membrane placed in vibratory communication with a modified labyrinth, or even an auditory capsule with an outer flexible covering, would be useless to the greater number of insects, for several reasons. First, such an apparatus, unless occupying a large proportion of the volume of an insect, would not present surface enough for this kind of receptor of vibrations; and secondly, the minuteness of such a membrane would render it impossible to covibrate with those sounds which generally occur in nature, and which the insects themselves can produce. Similarly, all non-aquatic vertebrates have an inner ear formed so as to bring the aërial vibrations which strike the tympanic membrane to bear with the greatest effect on the auditory nerve-filaments*; and the minuteness of insects also precludes this condition. Finally, the hard test, characteristic of the articulates, sets aside the idea that they receive the aërial vibrations through the covering of their bodies, like fishes, whose bodies are generally not only larger and far more yielding, but are also immersed in water which transmits vibrations with $4\frac{1}{4}$ times the velocity of the same pulses in air and with a yet greater increase in intensity. For these reasons I imagine that those articulates which are sensitive to sound and also emit characteristic sounds, will prove to possess receptors of vibrations external to the general surface of their bodies, and that the proportions and situation of these organs will comport

* See Section 4 of this paper.

with the physical conditions necessary for them to receive and transmit vibrations to the interior ganglia.

Naturalists, in their surmises as to the positions and forms of the organ of hearing in insects, have rarely kept in view the important consideration of those physical relations which the organ must bear to the aërial vibrations producing sound, and which we have already pointed out. The mere descriptive anatomist of former years could be satisfied with his artistic faculty for the perception of form; but the student of these days can only make progress by constantly studying the close relations which necessarily exist between the minute structure of the organs of an animal and the forces which are acting in the animal, and which traverse the medium in which the animal lives. The want of appreciation of these relations, together with the fact that many naturalists are more desirous to describe many new forms than to ascertain the function of one well-known form which may exist in all animals of a class, has tended to keep many departments of natural history in the condition of mere descriptive science. Those who are not professed naturalists appreciate this perhaps more than the naturalists themselves, who are imbued with that enthusiasm which always comes with the earnest study of any one department of nature; for the perusal of those long and laboriously precise descriptions of forms of organs without the slightest attempt, or even suggestion, as to their uses, affects a physicist with feelings analogous to those experienced by one who peruses a well-classified catalogue descriptive of physical instruments, while of the uses of these instruments he is utterly ignorant.

The following views, taken from the 'Anatomy of the Invertebrata' by C. Th. v. Siebold, will show how various are the opinions of naturalists as to the location and form of the organs of hearing in the Insecta:—"There is the same uncertainty concerning the organs of audition (as concerning the olfactory organs). Experience having long shown that most insects perceive sounds, this sense has been located sometimes in this and sometimes in that organ. But in their opinion it often seems to have been forgotten, or unthought of, that there can be no auditory organ without a special auditory nerve which connects directly with an acoustic apparatus capable of receiving, conducting, and concentrating the sonorous undulations. (The author who has erred most widely in this respect is Mr. L. W. Clarke in *Mag. Nat. Hist.*, September 1838, who has described at the base of the antennæ of *Carabus nemoralis*, Illig., an auditive apparatus composed of an auricula, a meatus auditorius externus and internus, a tympanum and labyrinthus, of all of

which there is not the least trace. The two white convex spots at the base of the antennæ of *Blatta orientalis*, and which Treviranus has described as auditory organs, are, as Burmeister has correctly stated, only rudimentary accessory eyes. Newport and Goureaux think that the antennæ serve both as tactile and as auditory organs. But this view is inadmissible, as Erichson has already stated, except in the sense that the antennæ, like all solid bodies, may conduct sonorous vibrations of the air; but even admitting this view, where is the auditory nerve? for it is not at all supposable that the antennal nerve can serve at the same time the function of two distinct senses.)

"Certain Orthoptera are the only Insecta with which there has been discovered in these later times a single organ having the conditions essential to an auditory apparatus. This organ consists, with the Acrididæ, of two fossæ or conchs, surrounded by a projecting horny ring, and at the base of which is attached a membrane resembling a tympanum. On the internal surface of this membrane are two horny processes, to which is attached an extremely delicate vesicle filled with a transparent fluid and representing a membranous labyrinth. This vesicle is in connexion with an auditory nerve which arises from the third thoracic ganglion, forms a ganglion on the tympanum, and terminates in the immediate neighbourhood of the labyrinth by a collection of cuneiform staff-like bodies with very finely pointed extremities (primitive nerve-fibres?), which are surrounded by loosely aggregated ganglionic globules. (This organ has been taken for a soniferous apparatus by Latreille. J. Müller was the first who fortunately conceived that with *Gryllus hieroglyphus* this was an auditory organ. He gave, however, the interpretation only as hypothetical; but I have placed it beyond all doubt by careful researches made on *Gomphoceros*, *Edipoda*, *Podisma*, *Caloptenus*, and *Truxalis*.)

"The Locustidæ and Achetidæ have a similar organ situated in the fore legs directly below the coxo-tibial articulation. With a part of the Locustidæ (*Meconema*, *Barbitistes*, *Phanoptera*, *Phylloptera*), there is on each side of this point a fossa, while with another portion of this family there are at this same place two more or less spacious cavities (auditory capsules) provided with orifices opening forward. These fossæ and these cavities have each on their internal surface a long-oval tympanum. The principal trachean trunk of the leg passes between two tympanums, and dilates at this point into a vesicle whose upper extremity is in connexion with a ganglion of the auditory nerve. This last arises from the first thoracic ganglion, and accompanies the principal nerve of the leg. From the ganglion in question passes off a band of ner-

vous substance which stretches along the slightly excavated anterior side of the trachean vesicle. Upon this band is situated a row of transparent vesicles containing the same kind of cuneiform staff-like bodies, mentioned as occurring with the Acrididæ. The two large trachean trunks of the fore legs open by two wide infundibuliform orifices on the posterior border of the prothorax; so that here, as with the Acrididæ, a part of this trachean apparatus may be compared to a tuba Eustachii. With the Achetidæ there is on the external side of the tibia of the fore legs an orifice closed by a white silvery membrane (tympanum), behind which is an auditory organ like that just described. (With *Acheta achatina* and *italica* there is a tympanum of the same size on the internal surface of the legs in question; but it is scarcely observable with *A. sylvestris*, *A. domestica*, and *A. campestris*.)"

Other naturalists have placed the auditory apparatus of diurnal Lepidoptera in their club-shaped antennæ, of bees at the root of their maxillæ, of *Melolontha* in their antennal plates, of *Locusta viridissima* in the membranes which unite the antenna with the head.

I think that Siebold assumes too much when he states that the existence of a tympanic membrane is the only test of the existence of an auditory apparatus. It is true that such a test would apply to the non-aquatic vertebrates; but their homologies do not extend to the articulates; and besides, any physicist can not only conceive of, but can actually construct other receptors of ærial vibrations, as I will soon show by conclusive experiments. Neither can I agree with him in supposing that the antennæ are only tactile organs; for very often their position and limited motion would exclude them from this function*; and moreover it has never been proved that the antennæ, which differ so much in their forms in different insects, are always tactile organs. They may be used as such in some insects; in others they may be organs of audition; while in other insects they may, as Newport and Goureau surmise, have both functions; for even granting that Müller's law of the specific energy of the senses extends to the insects, yet the anatomy of their nervous system is not sufficiently known to prevent the supposition that there may be two distinct sets of nerve-fibres in the antennæ or in connexion with their bases; so that the antennæ may serve both as tactile and as auditory organs—just as the hand, which receives at the same time the impression of the

* Indeed they are often highly developed in themselves while accompanied by *palpi*, which are properly placed, adequately organized, and endowed with a range of motion suitable to an organ intended for purposes of touch.

character of the surface of a body and of its temperature—or like the tongue, which at the same time distinguishes the surface, the form, the temperature, and the taste of a body. Finally, I take objection to this statement :—"Newport and Goureaux think that the antennæ serve both as tactile and as auditory organs. But this view is inadmissible, as Erichson has already stated, except in the sense that the antennæ, like all solid bodies, may conduct sonorous vibrations of the air." Here evidently Siebold had not in his mind the physical relations which exist between two bodies which give exactly the same number of vibrations; for it is well known that when one of them vibrates, the other will be set into vibration by the impacts sent to it through the intervening air. Thus if the fibrillæ on the antennæ of an insect should be tuned to the different notes of the sound emitted by the same insect, then when these sounds fell upon the antennal fibrils, the latter would enter into vibration with those notes of the sound to which they were severally tuned; and so it is evident that not only could a properly constructed antenna serve as a receptor of sound, but it would also have a function not possible in a membrane; that is, it would have the power of analyzing a composite sound by the covibration of its various fibrillæ to the elementary tones of the sound.

The fact that the existence of such an antenna is not only supposable, but even highly probable, taken in connexion with an observation I have often made in looking over entomological collections, viz. that fibrillæ on the antennæ of nocturnal insects are highly developed, while on the antennæ of diurnal insects they are either entirely absent or reduced to mere rudimentary filaments, caused me to entertain the hope that I should be able to confirm my surmises by actual experiments on the effects of sonorous vibrations on the antennal fibrillæ; also the well-known observations of Hensen* encouraged me to seek in aërial insects for phenomena similar to those he had found in the decapod the *Mysis*, and thus to discover in nature an apparatus whose functions are the counterpart of those of the apparatus with which I gave the experimental confirmation of Fourier's theorem, and similar to the supposed functions of the rods of the organ of Corti.

The beautiful structure of the plumose antennæ of the male *Culex mosquito* is well known to all microscopists; and these organs at once recurred to me as suitable objects on which to begin my experiments. The antennæ of these insects are twelve-jointed; and from each joint radiates a whorl of fibrils; and the latter gradually decrease in their lengths as we proceed from

* "Studien über das Gehörorgan der Decapoden," Siebold und Kolliker's *Zeitschrift für wissenschaftliche Zoologie*, vol. xiii.

those of the second joint from the base of the antenna to those of the second joint from the tip. These fibrils are highly elastic, and so slender that their lengths are over three hundred times their diameters. They taper slightly, so that the diameter at the base is to the diameter near the tip as 3 to 2.

I cemented a live male mosquito with shellac to a glass slide, and brought to bear on various fibrils a one-fifth objective. I then sounded successively, near the stage of the microscope, a series of tuning-forks with the openings of their resonant boxes turned towards the fibrils. On my first trials with an Ut_4 fork of 512 vibrations per second, I was delighted with the results of the experiments; for I saw certain of the fibrils enter into vigorous vibration, while others remained comparatively at rest.

The Table of experiments which I have given is characteristic of all of the many series which I have made. In the first column (A) I have given the notes of the forks in the French notation, which König stamps upon his forks. In the second (B) are the amplitudes of the vibrations of the end of the fibril in divisions of the micrometer-scale; and in column C are the values of these divisions in fractions of a millimetre.

A.	B.	C.
Ut_2 . .	0·5 div.	·0042 millim.
Ut_3 . .	2·5 „	·0200 „
Mi_3 . .	1·75 „	·0147 „
Sol_3 . .	2·0 „	·0168 „
Ut_4 . .	6·0 „	·0504 „
Mi_4 . .	1·5 „	·0126 „
Sol_4 . .	1·5 „	·0126 „
B_4^b . .	1·5 „	·0126 „
Ut_5 . .	2·0 „	·0168 „

The superior effect of the vibrations of the Ut_4 fork on the fibril is marked; but thinking that the differences in the observed amplitudes of the vibrations might be owing to differences in the intensities of the various sounds, I repeated the experiment, but vibrated the forks which gave the greater amplitudes of covibration with the lowest intensities; and although I observed an approach toward equality of amplitude, yet the fibril gave the maximum swings when Ut_4 was sounded; and I was persuaded that this special fibril was tuned to unison with Ut_4 or to some other note within a semitone of it. The differences of amplitude given by Ut_4 and Sol_3 and Mi_4 are considerable; and the Table also brings out the interesting observation that the lower (Ut_3) and the higher (Ut_5) harmonics of Ut_4 cause greater amplitudes of vibration than any intermediate notes. As long as a universal method for the determina-

tion of the relative intensities of sounds of different pitch remains undiscovered, so long will the science of acoustics remain in its present vague qualitative condition*. Now, not having the means of equalizing the intensities of the vibrations issuing from the various resonant boxes, I adopted the plan of sounding with a bow each fork with the greatest intensity I could obtain. I think that it is to be regretted that König did not adhere to the form of fork with *inclined prongs* as formerly made by Marloye; for with such forks one can always reproduce the same initial intensity of vibration by separating the prongs by means of the same cylindrical rod, which is drawn between them. Experiments similar to those already given revealed a fibril tuned to such perfect unison with Ut_3 that it vibrated through 18 divisions of the micrometer, or .15 millim., while its amplitude of vibration was only 3 divisions when Ut_4 was sounded. Other fibrils responded to other notes; so that I infer from my experiments on about a dozen mosquitos that their fibrils are tuned to sounds extending through the middle and next higher octave of the piano.

To subject to a severe test the supposition I now entertained, that the fibrils were tuned to various periods of vibration, I measured with great care the lengths and diameters of two fibrils, one of which vibrated strongly to Ut_3 , the other as powerfully to Ut_4 ; and from these measures I constructed in homogeneous pine-wood two gigantic models of the fibrils, the one corresponding to the Ut_3 fibril being about 1 metre long. After a little practice I succeeded in counting readily the number of

* I have recently made some experiments in this direction which show the possibility of eventually being able to express the intensity of an aërial vibration directly in fraction of Joule's dynamical unit, by measuring the heat developed in a slip of sheet rubber stretched between the prongs of a fork and enclosed in a compound thermo-battery. The relative intensities of the aërial vibration produced by the fork when engaged in heating the rubber and when the rubber is removed, can be measured by the method I described in the Philosophical Magazine, 1873, vol. xlv. p. 18. Of course, if we can determine the amount of heat produced per second by a known fraction of the intensity, we have the amount produced by the vibration with its entire intensity. Then means can be devised by which the aërial vibration produced by this fork can always be reproduced with the same intensity. This intensity, expressed in fraction of Joule's unit, is stamped upon the apparatus, which ever afterward serves as a true measure for obtaining the intensities of the vibrations of all simple sounds having the same pitch as itself. The same operation can be performed on other forks of different pitch; and so a series of intensities of different periods of vibration is obtained expressed in a corresponding series of fractions of Joule's unit. Recent experiments have given $\frac{1}{100000}$ of a Joule's unit as the approximate dynamic equivalent of ten seconds of aërial vibrations produced by an Ut_3 fork set in motion by intermittent electromagnetic action and placed before a resonator.

vibrations they gave when they were clamped at one end and drawn from a horizontal position. On obtaining the ratio of these numbers, I found that it coincided with the ratio existing between the numbers of vibrations of the forks to which covibrated the fibrils of which these pine-rods were models.

The consideration of the relations which these slender, tapering, and pointed fibrils must have to the aërial pulses acting on them, led me to discoveries in the physiology of audition which I imagine are entirely new. If a sonorous wave falls upon one of these fibrils so that its wave-front is at right angles to the fibril, and hence the direction of the pulses in the wave are in the direction of the fibril's length, the latter cannot be set in vibration; but if the vibrations in the wave are brought more and more to bear athwart the fibril, it will vibrate with amplitudes increasing until it reaches its maximum swing of covibration, when the wave-front is parallel to its length, and therefore the direction of the impulses on the wave are at right angles to the fibril. These curious surmises I have confirmed by many experiments made in the following manner. A fork which causes a strong covibration in a certain fibril is brought near the microscope, so that the axis of the resonant box is perpendicular to the fibril, and its opening is toward the microscope. The fibril in these circumstances enters into vigorous vibration on sounding the fork; but on moving the box round the stage of the microscope so that the axis of the box always points toward the fibril, the amplitudes of vibration of the fibril gradually diminish; and when the axis of the box coincides with the length of the fibril, and therefore the sonorous pulses act on the fibril in the direction of its length, the fibril is absolutely stationary, and even remains so when the fork in this position is brought quite close to the microscope. These observations at once revealed to me another function of these organs: for if, for the moment, we assume that the antennæ are really the organs which receive aërial vibrations and transmit them to an auditory capsule, or rudimentary labyrinth, then these insects must have the faculty of the perception of the direction of sound more highly developed than in any other class of animals. The following experiments will show the force of this statement, and at the same time illustrate the manner in which these insects determine the direction of a sonorous centre. I placed under the microscope a live mosquito, and kept my attention fixed upon a fibril which covibrated to the sound of a tuning-fork which an assistant placed in unknown positions around the microscope. I then rotated the stage of the instrument until the fibril ceased to vibrate, and then drew a line on a piece of paper under the microscope in the direction of the

fibril. On extending this line I found that it always cut within 5° of the position of the source of the sound. The antennæ of the male mosquito have a range of motion in a horizontal direction, so that the angle included between them can vary considerably inside and outside of 40° *; and I conceive that this is the manner in which these insects during night direct their flight toward the female. The song of the female vibrates the fibrillæ of one of the antennæ more forcibly than those of the other. The insect spreads the angle between his antennæ, and thus, as I have observed, brings the fibrillæ, situate within the angle formed by the antennæ, in a direction approximately parallel to the axis of the body. The mosquito now turns his body in the direction of that antenna whose fibrils are most affected, and thus gives greater intensity to the vibrations of the fibrils of the other antenna. When he has thus brought the vibrations of the antennæ to equality of intensity, he has placed his body in the direction of the radiation of the sound, and he directs his flight accordingly; and from my experiments it would appear that he can thus guide himself to within 5° of the direction of the female.

Some may assume (as I did when I began this research), from the fact of the covibration of these fibrils to sounds of different pitch, that the mosquito has the power of decomposing the sensation of a composite sound into its simple components, as is done by the higher vertebrates; but I do not hold this view, but believe that the range of covibration of the fibrils of the mosquito is to enable it to apprehend the ranging pitch of the sounds of the female. In other words, the want of definite and fixed pitch to the female's song demands for the receiving-apparatus of her sounds a corresponding range of covibration; so that, instead of indicating a high order of auditory development, it is really the lowest, except in its power of determining the direction of a sonorous centre, in which respect it surpasses by far our own ear†.

* The shafts of the antennæ include an angle of about 40° . The basal fibrils of the antennæ form an angle of about 90° , and the terminal fibrils an angle of about 30° , with the axis of the insect.

† Some physiologists, attempting to explain the function of the semicircular canals, assume, because these canals are in three planes at right angles to each other, that they serve to fix in space a sonorous centre, just as the geometrician by his three coordinate planes determines the position of a point in space. But this assumption is fanciful and entirely devoid of reason; for the semicircular canals are always in the same dynamic relation to the tympanic membrane which receives the vibration, to be transmitted always in one way through the ossicles to the inner ear. Really we determine the direction of a sound by the difference in the intensities of the effects produced in the two ears; and this determination is aided by the form of the outer ear, and by the fact that ~~man~~ can turn his head around

The auditory apparatus we have just described does not in the least confirm Helmholtz's hypothesis of the functions of the organ of Corti; for the supposed power of that organ to decompose a sonorous sensation depends upon the existence of an auditory nerve differentiated as highly as the covibrating apparatus, and in the case of the mosquito there is no known anatomical basis for such an opinion. In other words, my researches show external covibrating organs whose functions replace those of the tympanic membrane and chain of ossicles in receiving and transmitting vibrations; while Helmholtz's discoveries point to the existence of internal covibrating organs which have no analogy to those of the mosquito, because the functions of the former are not to receive and transmit vibrations to the sensory apparatus of the ear, but to give the sensation of pitch and to decompose a composite sonorous sensation into its elements; and this they can only do by their connexion with a nervous development whose parts are as numerous as those of the covibrating mechanism. Now, as such a nervous organization does not exist in insects, it follows that neither anatomical nor functional relations exist between the covibrating fibrils on the antennæ and the covibrating rods in the organ of Corti, and therefore that neither Hensen's observations on the *Mysis* (assumed by Helmholtz to confirm his hypothesis) nor mine on the mosquito can be adduced in support of Helmholtz's hypothesis of audition*.

The above-described experiments were made with care; and I think that I am authorized to hold the opinion that I have established a physical connexion existing between the sounds emitted by the female and the covibrations of the antennal fibrillæ of the male mosquito; but only a well-established physiological relation between these covibrating parts of the animal and the development of its nervous system will authorize us to state that these are really the auditory organs of the insect. At this stage of the investigation I began a search through the zoological journals, and found nearly all that I could desire in a paper in vol. iii. (1855) of the 'Quarterly Journal of the Micro-

a vertical axis. Other mammalia, however, having the axis of rotation of the head more or less horizontal, have the power of facilitating the determination of motion by moving the axis of their outer ears into different directions. It is also a fact that, when one ear is slightly deaf, the person unconsciously so affected always supposes a sound to come from the side on which is his good ear.

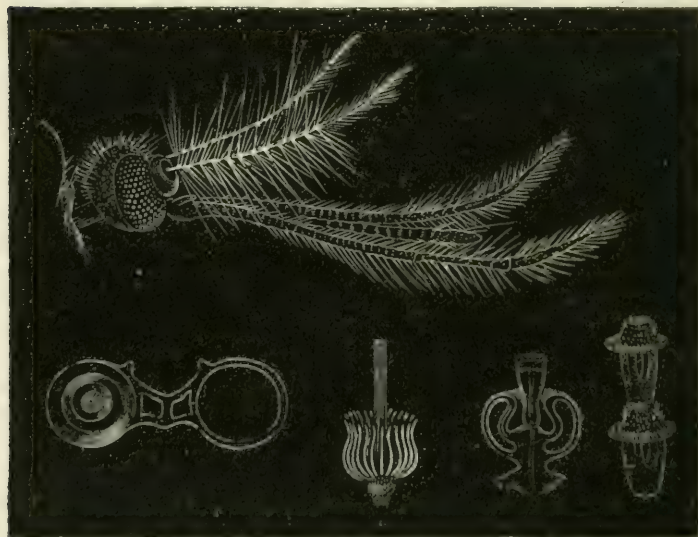
* Also, the organ of Corti having disappeared in the lower vertebrates, it is not likely that it would reappear in the Articulata; and especially will this opinion have weight when we consider that the peculiar function of the organ of Corti is the appreciation of those composite sounds whose signification mammals are constantly called upon to interpret.

scopical Society,' entitled "Auditory Apparatus of the *Culex mosquito*," by Christopher Johnston, M.D., Baltimore, U.S.

In this excellent paper I found clear statements showing that its talented author had surmised the existence of some of the physical facts which my experiments and observations have established*. To show that anatomical facts conform to the hypothesis that the antennal fibrils are the auditory organs of the mosquito, I cannot do better than quote the following from Dr. Johnston's paper:—

"While bearing in mind the difference between *feeling a noise* and *perceiving a vibration*, we may safely assume with Carus—for a great number of insects at least—that whenever true auditory organs are developed in them, their seat is to be found in the neighbourhood of the *antennæ*. That these parts themselves are in some instances concerned in collecting and transmitting sonorous vibrations, we hold as established by the observations we have made, particularly upon the *Culex mosquito*; while we believe, as Newport has asserted in general terms, that they serve also as tactile organs.

Fig. 2.



"The male mosquito differs considerably, as is well known, from the female, his body being smaller and of a darker colour,

* A short time before the death of my friend Professor Agassiz, he wrote me these words:—"I can hardly express my delight at reading your letter. I feel you have hit upon one of the most fertile mines for the elucidation of a problem which to this day is a puzzle to naturalists, the seat of the organ of hearing in Articulates."

and his head furnished with *antennæ* and *palpi* in a state of greater development (fig. 2). Notwithstanding the fitness of his organs for predatory purposes, he is timid, seldom entering dwellings or annoying man, but restricts himself to damp and foul places, especially sinks and privies. The female, on the other hand, gives greater extension to her flight, and, attacking our race, is the occasion of no inconsiderable disturbance and vexation during the summer and autumn months.

"The head of the male mosquito, about 0·67 millim. wide, is provided with lunate eyes, between which in front superiorly are found two pyriform capsules nearly touching each other, and having implanted into them the very remarkable *antennæ*.

"The capsule, measuring about 0·21 millim., is composed of a horny substance, and is attached posteriorly by its pedicle, while anteriorly it rests upon a horny ring united with its fellow by a transverse fenestrated band, and to which it is joined by a thin elastic membrane. Externally it has a rounded form, but internally it resembles a certain sort of lamp-shade with a constriction near its middle; and between this inner cup and outer globe there exists a space, except at the bottom or proximal end, where both are united.

"The *antennæ* are of nearly equal length in the male and the female.

"In the male the antenna is about 1·75 millim. in length, and consists of fourteen joints, twelve short and nearly equal, and two long and equal terminal ones, the latter measuring (together) 0·70 millim. Each of the shorter joints has a fenestrated skeleton with an external investment, and terminates simply posteriorly, but is encircled anteriorly with about forty *papillæ*, upon which are implanted long and stiff hairs, the proximal sets being about 0·79 millim. and the distal ones 0·70 millim. in length; and it is beset with minute bristles in front of each whorl.

"The two last joints have each a whorl of about twenty short hairs near the base.

"In the female the joints are nearly equal, number but thirteen, and have each a whorl of about a dozen small hairs around the base. Here, as well as in the male, the parts of the *antennæ* enjoy a limited motion upon each other, except the basal joint, which, being fixed, moves with the capsule upon which it is implanted.

"The space between the inner and outer walls of the capsule, which we term confidently the auditory capsule*, is filled with a fluid of moderate consistency, opalescent, containing minute spherical corpuscles, and which probably bears the same

* See fig. 2.

relation to the nerve as does the lymph in the scale of the cochlea of higher animals. The nerve itself of the antenna proceeds from the first or cerebral ganglion, advances toward the pedicle of the capsule in company with the large trachea, which sends its ramifications throughout the entire apparatus; and penetrating the pedicle, its filaments divide into two portions. The central threads continue forward into the antenna, and are lost there; the peripheral ones, on the contrary, radiate outward in every direction, enter the capsular space, and are lodged there for more than half their length in *sulci* wrought in the inner wall or cup of the capsule.

"In the female the disposition of parts is observed to be nearly the same, excepting that the capsule is smaller, and that the last distal antennal joint is rudimental.

"The proboscis does not differ materially in the two sexes; but the palpi, although consisting in both instances of the same number of pieces, are very unlike. In the female they are extremely short, but in the male attain the length of 2.73 millims., while the proboscis measures but 2.16 millims. They are curved upward at the extremity.

"... The position of the capsules strikes us as extremely favourable for the performance of the function which we assign to them; besides which there present themselves in the same light the anatomical arrangement of the capsules, the disposition and lodgment of the nerves, the fitness of the expanded whorls for receiving, and of the jointed antennæ fixed by the immovable basal joint for transmitting vibrations created by sonorous undulations. The intracapsular fluid is impressed by the shock, the expanded nerve appreciates the effect of the sound by the quantity of the impression, of the pitch or quality by the consonance of particular whorls of stiff hairs according to their lengths, and of the direction in which the undulations travel by the manner in which they strike upon the antennæ or may be made to meet either antenna in consequence of an opposite movement of that part.

"That the male should be endowed with superior acuteness of the sense of hearing appears from the fact that he must seek the female for sexual union either in the dim twilight or in the dark night, when nothing but her sharp humming noise can serve him as a guide. The necessity for an equal perfection of hearing does not exist in the female; and accordingly we find that the organs of the one attain a development which the other's never reach. In these views we believe ourselves to be borne out by direct experiment, in connexion with which we may allude to the greater difficulty of catching the male mosquito.

"In the course of our observations we have arrived at the

conclusion that the antennæ serve to a considerable extent as organs of touch in the female; for the palpi are extremely short, while the antennæ are very movable and nearly equal the proboscis in length. In the male, however, the length and perfect development of the palpi would lead us to look for the seat of the tactile sense elsewhere; and in fact we find the two apical antennal joints to be long, movable, and comparatively free from hairs, and the relative motion of the remaining joints very much more limited."

My experiments on the mosquito began late in the fall; and therefore I was not able to extend them to other insects. This spring I purpose to resume the research, and will experiment especially on those Orthoptera and Hemiptera which voluntarily emit distinct and characteristic sounds.

[To be continued.]

LIII. *On the Action of Solids and of Friction in liberating Gas from Solution.* By CHARLES TOMLINSON, F.R.S.*

IN the Philosophical Magazine for April 1873, I stated that chemically clean solid bodies, in their behaviour towards gaseous solutions, admit of being arranged into four classes or groups. The first group includes glass and all vitrified and siliceous surfaces, and the denser metals, including mercury. The gaseous solution, whether supersaturated or not, adheres to the chemically clean surfaces of these bodies in the most perfect manner, so that there is no separation of gas.

The second group includes oils, both fixed and volatile; fatty bodies whether acid or neutral, various kinds of wax, resin and gum-resin, camphor, spermaceti and similar bodies, not soluble in water. The surfaces of such bodies, though chemically clean, liberate gases from their aqueous solutions; and they do so the more efficiently, in proportion as their surfaces are less liable to be wetted by the water of the solution. In other words, the gas adheres to such surfaces with greater force than the water.

When it is said that a body in Class I., not chemically clean, liberates gas from solution, it is contaminated more or less with one of the substances in Class II.

The third class consists of porous solids which are eminently active in liberating gas from solution. This class includes woods of all kinds, hard and soft, and the charcoals made from them; also coal, coke, anthracite, jet, plumbago, roll sulphur, pumice, meerschaum, bone, ivory, chalk, lime, indigo, and the less dense metals, such as aluminium and magnesium; and lamellar metals

* Communicated by the Author.

such as antimony, and crystalline metals such as bismuth, which contain a good deal of occluded gas. The strong adhesion between the gases and the pores of bodies in this class may be strikingly exhibited by placing one of them in soda-water, when it becomes apparently saturated with the gas, whilst more gas seeking to precipitate itself upon the innumerable surfaces already occupied gives rise to the appearance of a stream of gas constantly ascending from the porous surface until the liquid seems to be exhausted; but the action may be renewed by reducing the pressure or raising the temperature. In the latter case, when the gas is expelled and the liquid is at or near its boiling-point, it is constituted exactly like the soda-water, which may be called an aqueous supersaturated solution of CO_2 , while the other may be termed an aqueous supersaturated solution of steam*. A strip of aluminium ($1\frac{3}{4}$ inch by $\frac{3}{4}$ inch) cleaned by being rubbed between two corks in the strongest oil of vitriol, and rinsing in water, was active in disengaging gas from soda-water; and when taken out and put into water over a spirit-lamp, it liberated such copious streams of vapour, when the water boiled, as to be supported vertically on one of its long edges, while innumerable bubbles issued from both sides, and continued to do so for about a minute after the lamp had been removed. When taken out and cooled in cold water and again transferred to soda-water, it was as active as before in occluding and liberating gas.

The fourth class of bodies are those which are soluble in water, and act by lessening the adhesion between the gas and the water, as when powdered white sugar is put into a glass of sparkling Moselle wine. A piece of gamboge in soda-water is also a good example of this class, while bodies so little soluble as phosphorus and iodine belong to it.

The results obtained with all four classes of solids in their action on soda-water may, with proper precautions, be obtained with aqueous solutions of ammonia, of hydrochloric acid, of chlorine, and of nitrous oxide, and also with liquids at or near their boiling-points.

Next as to the effect of friction. It has long been known to chemists that certain saline solutions, which show no disposition to deposit crystals, may be started into crystalline action by rubbing the inside of the vessel below the level of the solution with a glass rod. This effect is produced although every part of the arrangement be chemically clean; and it has not, so far as I

* This definition is given in a paper read before the Royal Society, January 21, 1869 (Proc. Roy. Soc. vol. xvii. p. 240), "On the Action of Solid Nuclei in liberating Vapour from Boiling Liquids." I may state that Professor Schrötter (Pogg. Ann. vol. cxxxvii.) accepts this definition.

know, been explained. It is the same with a gaseous or vaporous solution. Soda-water, in a chemically clean test-glass, in which not a bubble of gas is visible, will display a line of bubbles along the path described with friction by a clean glass, metal, or other rod, against the inside of the glass below the level of the liquid. So also, if a glass rod be rubbed against the side of a vessel containing a liquid (such as spirits of wine, or a saline solution such as one of common salt) at or near the boiling-point after the source of heat has been removed, bubbles of vapour may be liberated so abundantly that the liquid may be made not only to boil, but to boil over.

A gentle rubbing sometimes fails to convert a friction line into a bubble line, whereas harder rubbing and a quicker motion produce the effect. And in general hard bodies are more efficacious in producing the result than soft ones. It is not necessary that the friction produce an actual scratching of the surface; nor does the track of a bubble line remain more sensitive than other parts in liberating gas after the first display is over. The more highly supersaturated or superheated the solution the more sensitive the surface appears to be, and the smaller the amount of friction required. Beer gently warmed produces foam by rubbing the side of the vessel. Or if, instead of rubbing the side of the vessel, two solids such as copper and steel be introduced into the liquid and rubbed against each other, bubbles are produced.

An explanation of these interesting facts, offered by Professor Schrötter (*loc. cit.*), is to the effect that the friction immediately produces a change of mechanical action into latent heat or work.

Some of my scientific friends to whom I have showed these effects, endeavour to explain them by supposing that the friction produces heat, or electricity, or some molecular change on the surface of the vessel.

My explanation is derived from a more vulgar source. From the shelter of an archway during a heavy fall of rain I have watched the extemporized puddles before me and admired the large bubbles of air which frequently follow those drops which plunge into the miniature lake with something like decision of character. Old Mariotte was interested in the same phenomenon. He says, "Each drop of rain, in falling from the height of the cloud, drags with it two or three times as much air as its own size, as may be shown by letting a little ball of lead fall into a bucket of water; for as soon as it touches the bottom two or three bubbles of air rise, each as large as itself, which can only proceed from air which follows it to the bottom of the vessel." He then refers to the *trompe*, in which air is dragged down by water*.

* *Œuvres*, 1717, vol. ii. p. 353.

Mariotte's experiment, differently arranged, forms a good illustration for a class. Over a tall glass cylinder of water is suspended a funnel with its beak from 20 to 25 inches above the axis of the water-jar. A shot put into the funnel will thus be delivered neatly and properly to the water, and as soon as it strikes the bottom a number of bubbles of air are liberated, some say twenty times (Mr. Rodwell informs me thirteen times) the volume of the shot*. The old idea (still retained in some modern books) was that the air thus liberated was air adhering to the shot; to disprove which I oiled some shot and dropped them in with the same result.

The more rational explanation is that the shot, in plunging into the water, displaces a quantity of that fluid in the form of a well or cylindrical shaft, to which the shot forms the lower boundary, and into which air, as the more mobile body, rushes before the water has time to close over it; and as the shot is still pursuing its journey, it makes a path which the cylinder of air continues to follow until both are arrested at the bottom of the vessel, where they are disposed of according to their respective densities and that of the surrounding medium.

Now to apply this to the liberation of gas from soda-water, &c. by the friction of a hard body against the side of the vessel. The glass rod or the steel knitting-needle, on being pressed against the side of the glass, displaces a certain small quantity of the liquid, and on moving the solid, with friction, against the side successive quantities of liquid are thus displaced. A certain time, however short, must elapse before the water can fairly close in upon the moving points of the line thus traced; but however quick the water may be in filling up the void, the gas is quicker, and hence a friction line becomes a line of bubbles.

The same explanation applies to other gaseous solutions, to solutions of vapour, such as spirits of wine at or near its boiling-point, and also to certain saline solutions. The gas, or the vapour, or the salt, fills up the spaces in the friction line more quickly than the liquid part of the solution can do; and thus we have a line of gas- or of vapour-bubbles, or a line of minute crystals.

Highgate, N. October 1874.

* See Magnus, *Pogg. Ann.* vol. xcv.

LIII. *On the Surface-Forces caused by the Communication of Heat.*
By Professor OSBORNE REYNOLDS.*

IN a paper read before the Royal Society, June 18, I pointed out, as it seemed to me, that whenever evaporation or condensation takes place on a surface they are attended with certain forces tending respectively to drive the surface back and urge it forward, these forces arising, according to the kinetic theory, from the momentum which is imparted from the surface to the particles driven off, and *vice versâ*. I also pointed out at the end of the paper that similar effects will be produced whenever heat is communicated from a surface to a gas, and *vice versâ*. The possibility of this latter effect only occurred to me as I was on the point of sending off the paper, and consequently was added by way of an appendix. The first part of the paper contains a description of some experiments undertaken to verify my conclusions respecting the forces of evaporation and condensation, the results of which seem to me to be fully explained by these forces; so that had I rewritten the paper after becoming aware of the possible existence of the other force, I should have had nothing to add in connexion with these experiments. I had, however, also endeavoured to show that the first class of forces afforded an explanation of Mr. Crookes's experiments; and had this part of the paper been rewritten it would have been somewhat altered, as the last class of forces (those arising from the simple communication of heat) seem to afford a simpler explanation of some of the phenomena observed by Mr. Crookes. I regret that this was not done, as, from some remarks in a paper published in the August Number of the Philosophical Magazine, I fear that Mr. Crookes has not understood my meaning, and has consequently been at the trouble of making further experiments, which, however valuable from other considerations, throw no fresh light on the case in point. However, before proceeding to discuss the subject further, I would set myself straight with Mr. Crookes in one or two particulars.

Mr. Crookes appears to complain that I did not give him credit for having obtained evidence of repulsion by heat in a medium as dense as that which I used, viz. from $\frac{1}{2}$ to $\frac{3}{4}$ inch of mercury. Now the only account of his experiments which I had seen was the abstract published in the 'Proceedings of the Royal Society,' December 1; and in this the highest pressure at which he definitely states he obtained repulsion is 3 millimetres, or one tenth of an inch: but this in truth was not the point. In art. 44 of his paper he describes an experiment in which he did not

* Communicated by the Author.

obtain repulsion until the Sprengel pump had been at work for a long time after the gauge showed half a millimetre. It was the results of this experiment which I was endeavouring to explain, and consequently it was to this experiment that my remarks applied; and I had not the least intention of implying that these were the only results which Mr. Crookes obtained. However, had it not been so, had I misread Mr. Crookes's paper as he supposed, I think that he would have forgiven me when he sees that he has committed a similar offence against me. He commences his remarks on my paper by saying, "In my exhausted receiver he assumes the presence of aqueous vapour;" whereas nowhere in my paper do I mention any such assumption, nor did it enter into my head to make it. Nay, further, I think I have shown, however darkly, that, under the conditions under which Mr. Crookes's experiments were made, aqueous vapour would not be sufficient to explain the results, since it would be to all intents a non-condensable gas. However, enough of this.

So far as I can see, the case now stands thus:—

1. Whenever a body is surrounded by a condensable medium (that is, vapour at its point of saturation), heating or cooling of the body will be respectively attended with evaporation and condensation, and hence with forces over the surface which changes temperature.

2. The amount of evaporation or condensation will not depend on the density of the vapour with which the surface is surrounded, provided only that it be at its point of saturation, but will depend on the amount of heat available; that is to say, it will depend on the amount of heat imparted to or taken from the body. Thus the evaporation of mercury would take place as readily in a medium of too small density to be measured as the evaporation of water under the pressure of $\frac{3}{4}$ of an inch.

3. The presence of a non-condensable gas will greatly retard the rate of evaporation and condensation.

4. That under the conditions (1), there will be forces arising from convection-currents in the surrounding medium, which will generally act in opposition to the forces (1), but which will diminish with the density of the medium, while the other forces remain constant and therefore must ultimately prevail.

5. That there is yet another set of forces, which act when the medium is not in a state of saturation, *i. e.* is not condensable. These forces arise from the communication of heat to or from the surface from or to the gas. These forces will be directly proportional to the rate at which the heat is communicated; and since this rate has been shown by Professor Maxwell to be independent of the density of the gas, these forces, like those arising from condensation and evaporation, will be independent of the

density of the surrounding medium, and their effect will increase as the density and convection-currents diminish.

These forces would appear, if their magnitude is sufficient, to afford an explanation of all Mr. Crookes's results if the medium is not in a state of saturation; but when, as in my experiments, the medium is steam, and water is present in the receiver, or, as I suppose in Mr. Crookes's experiments, mercury was present, and the medium was vapour of mercury, or at any rate sulphuric acid, then it would be impossible for the medium to communicate heat to the ball or surface without condensation; and hence in such cases it seems to me that the effects must be due to the forces of condensation.

LIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 309.]

March 19, 1874.—Joseph Dalton Hooker, C.B., President, in the Chair.

THE following communications were read:—

“Preliminary Notice of Experiments concerning the Chemical Constitution of Saline Solutions.” By Walter Noel Hartley, F.C.S.

The author has been engaged in investigating the above subject during the last eighteen months; and his experiments being still in progress, he thinks it desirable to place the following observations on record.

In the examination of the absorption-spectra, as seen in wedge-shaped cells, of the principal salts of cerium, cobalt, copper, chromium, didymium, nickel, palladium, and uranium, to the number of nearly sixty different solutions, it was noticed that the properties of the substances in regard to changes of colour could be ascertained by noticing the absorption-curves and bands, so that, provided water be without chemical action, it could be foreseen what change would occur on dilution of a saturated solution.

The effect of Heat on Absorption-spectra.

When saturated solutions of coloured salts are heated to 100° C., 1st, there are few cases in which no change is noticed. 2ndly, generally the amount of light transmitted is diminished to a small extent by some of the more refrangible, the less refrangible or both kinds of rays being obstructed. 3rdly, there is frequently a complete difference in the nature of the transmitted light. Anhydrous salts not decomposed, hydrated compounds not dehydrated at 100° C., and salts which do not change colour on dehydration, give little or no alteration in their spectra when heated.

Solutions of hydrated salts, and most notably those of haloid

compounds, do change; and the alteration is, if not identical with, similar to that produced by dehydration and the action of dehydrating liquids, such as alcohol, acids, and glycerine, on the salts in crystals or solution.

A particular instance of the action of heat on an aqueous solution is that of cobalt chloride, which gives a different series of dark bands in the red part of the spectrum at different temperatures, ranging between 23° C. and 73° C. Band after band of shadow intercepts the red rays as the temperature rises, till finally nothing but the blue are transmitted. Drawings of six different spectra of this remarkable nature have been made. The changes are most marked between 33° and 53° , when the temperature may be told almost to a degree by noting the appearance of the spectrum. Though to the unaided eye cobalt bromide appears to undergo the same change, yet, as seen with the spectroscope, it is not of so curious a character, the bands being not so numerous.

With cobalt iodide a band of red light is transmitted at low temperatures; the band of light moves towards the opposite end of the spectrum with rise of temperature, until it is transferred to such a position that it consists of green rays only. In this instance the change to the eye is more striking when seen without the spectroscope, because the mixtures of red, yellow, and green rays, which are formed during the transition, give rise to very beautiful shades of brown and olive-green. Thus a saturated solution at 16° C. was of a brown colour; at -10° C. it became of a fiery red and crystals separated, at $+10^{\circ}$ reddish brown, at 20° the same, at 35° vandyke brown, at 45° a cold brown tint with a tinge of yellowish green, at 55° a decidedly yellowish green in thin layers and yellow-brown in thick, at 65° greenish brown, thin layers green, and at 75° olive-green. An examination of this cobalt salt has shown that there are two distinct crystalline hydrates: the one, formed at high temperatures, has the formula $\text{CoCl}_2 \cdot 2\text{H}_2\text{O}$, and is of a dark green colour; the other, which contains a much larger proportion of crystalline water, $\text{CoCl}_2 \cdot 6\text{H}_2\text{O}$, is produced at a low temperature, and its colour is generally brown, in cold weather inclining to red.

The action of heat on solutions of didymium is characterized by a broadening of the black lines seen in the spectrum, more especially of the important band in the yellow; and in the case of potassio-didymium nitrate this is accompanied by the formation of a new line. In the case of didymium acetate, which decomposes with separation of a basic salt, the lines thickened on heating.

Thermo-chemical Experiments.

Regnault (Institut, 1864; Jahresbericht, 1864, p. 99) has shown that on diluting a saturated solution of a salt, as a rule there is an absorption of heat; but in one or two cases he noticed that heat was evolved. The change in colour that takes place on the dilution of saturated solutions of cobalt iodide, cupric chloride, bromide, and acetate is very remarkable. There is every likelihood that this phenomenon is due in each case to the formation of a liquid hydrate.

It is impossible of belief that accompanying such a circumstance there should be no measurable development of heat; and the author's experiments have proved that in the above cases, at any rate, the heat disengaged is very considerable—amounting, for instance, on the part of cupric chloride, at least to about 2565 units when 1 gram molecule of the crystalline salt is dissolved in its minimum of water at 16° C. and brought into contact with sufficient to make the addition of 40 Aq. These numbers only roughly approximate to the truth. On diluting a solution of cobalt iodide till the red colour appears, the thermal effect must be much greater, as not only does it register several degrees on an ordinary thermometer, but it may be perceived by the hand.

The conclusions indicated by these results are obvious, but it is beyond the scope of this paper to refer to them. The writer hopes before long to complete his experiments with the view of having them communicated to the Royal Society.

“On the Attraction of Magnets and Electric Conductors.” By George Gore, F.R.S.

Being desirous of ascertaining whether, in the case of two parallel wires conveying electric currents, the attractions and repulsions were between the currents themselves or the substances conveying them, and believing this question had not been previously settled, I made the following experiment:—

I passed a powerful voltaic current through the thick copper wire of a large electromagnet, and then divided it equally between two vertical pieces of thin platinum wire of equal diameter and length (about six or seven centimetres), so as to make them equally white-hot, the two wires being attached to two horizontal cross wires of copper.

On approaching the two vertical wires symmetrically towards the vertical face of one pole of the horizontally placed magnet, and at equal distances from it, so that the two downward currents in them might be equally acted upon by the downward and upward portions respectively of the currents which circulated round the magnet-pole, the one was strongly bent towards and the other from the pole, as was, of course, expected; but not the least sign of alteration of relative temperature of the two wires could be perceived, thereby proving that not even a small proportion of the current was repulsed from the repelled wire or drawn into the attracted one, as would have occurred had the attraction and repulsion taken place, even to a moderate degree, between the currents themselves; and I therefore conclude that *the attractions and repulsions of electric conductors are not exerted between the currents themselves, but between the substances conveying them.*

Some important consequences appear to flow from this conclusion, especially when it is considered in connexion with Ampère's theory of magnetism, and with the molecular changes produced in bodies generally by electric currents and by magnetism.

As every molecular disturbance produces an electric alteration in

bodies, so, conversely, the discoveries of numerous investigators have shown that every electric current passing near or through a substance produces a molecular change, which is rendered manifest in all metals, liquid conductors, and even in the voltaic arc by the development of sounds, especially if the substances are under the influence of two currents at right angles to each other. In iron it is conspicuously shown also by electrotorsion, a phenomenon I have found and recently made known in a paper read before the Royal Society.

Numerous facts also support the conclusion that the molecular changes referred to last as long as the current. De la Rive has shown that a rod of iron, either transmitting or encircled by an electric current, emits, as long as the current lasts, a different sound when struck; and we know it also exhibits magnetism. The peculiar optical properties of glass and other bodies with regard to polarized light discovered by Faraday also continue as long as the current. A rod of iron also remains twisted as long as it transmits and is encircled by electric currents; and in steel and iron the molecular change (like magnetism) partly remains after the currents cease, and enables the bar to remain twisted.

That the peculiar molecular structure produced in bodies generally by the action of electric currents also possesses a definite direction with regard to that of the current, is shown by the rigidly definite direction of action of magnetized glass and many other transparent bodies upon polarized light, also by the difference of conductivity for heat and for electricity in a plate of iron parallel or transverse to electric currents, by the stratified character of electric discharges in rarefied gases and the action of electric currents upon it, and especially by the phenomena of electrotorsion. In the latest example an upward current produces a reverse direction of twist to a downward one, and a right-handed current develops an opposite torsion to a left-handed one; and the two latter are each internally different from the former. As each of these four torsions is an outward manifestation of the collective result of internal molecular disturbance and possesses different properties, these four cases prove the existence of four distinct molecular movements and four corresponding directions of structure; and the phenomena altogether are of the most rigidly definite character.

As an electric current imparts a definite direction of molecular structure to bodies, and as the attractions and repulsions of electric wires are between the wires themselves and not between the currents, repulsion instead of attraction must be due to *difference of direction of structure* produced by difference of direction of the currents.

Although the Ampèrean theory has rendered immense service to magnetic science, and agrees admirably with all the phenomena of electromagnetic attraction, repulsion, and motion, it is in some respects defective; it assumes that magnetism is due to innumerable little electric currents continually circulating in one uniform direction round the molecules of the iron; but there is no known

instance of electric currents being maintained without the consumption of power, and in magnets there is no source of power; electric currents also generate heat, but a magnet is not a heated body.

If, however, we substitute the view that the phenomena of attraction and repulsion of magnets are due, not to continuously circulating electric currents, but (as in electric wires) to definite directions of molecular structure, such as is shown by the phenomena of electrotorsion to really exist in them, the theory becomes more perfect. It would also agree with the fact that iron and steel have the power of retaining both magnetism and the electrotorsional state after the currents or other causes producing them have ceased.

According to this view, a magnet, like a spring, is not a source of power, but only an arrangement for storing it up, the power being retained by some internal disposition of its particles acting like a "ratchet" and termed "coercive power." The fact that a magnet becomes warm when its variations of magnetism are great and rapidly repeated, does not contradict this view, because we know it has then, like any other conductor of electricity, electric currents induced in it, and these develop heat by conduction-resistance.

According also to this view, any method which will produce the requisite direction of structure in a body will impart to it the capacity of being acted upon by a magnet; and any substance, ferruginous or not, which possesses that structure has that capacity; and, in accordance with this, we find that a crystal of cyanite (a silicate of alumina) possesses the property, whilst freely suspended, of pointing north and south by the directive influence of terrestrial magnetism, and one of stannite (oxide of tin) points east and west under the same conditions.

LV. Intelligence and Miscellaneous Articles.

ON THE TEMPERATURE OF THE SUN. BY J. VIOLLE.

1. **P**URSUING my researches on the effective temperature of the sun as I have previously defined it (see the *Philosophical Magazine* for August and September, pp. 158 and 233), I effected a number of measurements in an enclosure at a high temperature. These experiments appeared to me to present a special interest in consequence of the affirmation, first put forth by Mr. Waterston, and afterwards supported by Father Secchi, that the excess of temperature received by a thermometer under the action of the sun is independent of the temperature of the enclosure in which the thermometer is placed—whence this consequence, that the 200° or 250° which we have varied the temperature of the enclosure "without observing the least difference, either in the amount of the excess or in the time employed in attaining it," are only an insignificant fraction of the temperature of the sun. It is, however, easy to demon-

strate, starting from the fundamental equation

$$\alpha^{\theta} - \alpha^t = \frac{\omega}{s} \alpha^x,$$

that, if the effective temperature x of the sun is sufficiently elevated for a variation of 250° in the temperature t of the enclosure not to affect the quantity of the excess $\theta - t$, the constant value of the excess must be infinitely greater than any of those which have been observed. Putting aside the hypothesis of $x = \infty$, the equation above recalled shows that $\theta - t$ must diminish continuously in proportion as the temperature of the enceinte rises, the law of this diminution being represented by the formula

$$\theta - t = \frac{c}{\alpha^t},$$

where c is a constant the value of which is proportional to the intensity α^x of the radiation at the time of the experiment.

This diminution of $\theta - t$ with the rise of the temperature of the enceinte has been verified in the experiments which I have made with a large actinometer constructed essentially like the small one described in my last communication, with only the modifications necessary in order to operate at a high temperature. This is shown by the following Table, in which I have also inscribed the values of α^x and x deduced from each observation by the method of calculation previously explained.

		θ .	t .	$\theta - t$.	Corrected excess of cooling.	α^x .	x .
	h m						
Aug. 12. ...	1 0	116 10	99 35	16 75	16 18	49100	1407 ^e
„ 31. ...	1 45	116 25	107 40	9 55	14 17	45700	1399
Sept. 5. ...	1 10	125 40	116 15	9 25	13 79	47600	1404
Aug. 26. ...	1 0	144 72	136 50	8 22	12 15	48910	1408

II. Experiments made at divers altitudes have led me to represent the intensity α^x , of the solar radiation enfeebled by its passage through our atmosphere, by

$$\alpha^x = \alpha^x_p \frac{H + bf}{\cos z},$$

α^x being the true intensity of the solar radiation before passing through our atmosphere, H the atmospheric pressure at the place of the experiment, f the value, in millimetres, of the actual tension of the aqueous vapour, z the zenith-distance of the sun, p and b constants, $p = 0.9989$, and $b = 7.8$.

The following Table exhibits the accordance of the formula with experiment. The last column contains the values of x deduced directly from observation; the penultimate column gives the values

of x calculated by means of the preceding formula, adopting

$$\alpha^x = 143400.$$

			metres.	H. mil- lims.	f. mil- lims.	$\epsilon = \frac{H}{\cos z}$	$\frac{H+bf}{\cos z}$	α^x	x .	
									Calc.	Obs.
	h m									
July 4.	12 0	Seyssinet ...	(213)	748.8	19.07	808	969	49140	1408	1407
"	"	Moucherotte. (1906)		613.5	13.82	662	779	60535	1435	1435
July 5.	"	Seyssinet ...	(213)	749.0	17.69	811	960	49620	1410	1410
Sept. 3.	11 0	"	"	746.0	17.19	969	1142.9	40540	1383	1383
"	"	Moucherotte. (1906)	"	611.0	6.53	793	858.5	55460	1424	1424
May 2.	4 43	Galibier	(2653)	553.1	3.30	1340	1402	30550	1346	1347

From the value adopted for α^x we deduce

$$X = 1550^\circ.$$

Such is therefore the *effective temperature* of the sun, correction being made for the influence of the atmosphere.

III. I essayed, finally, to determine the *true* mean temperature of the solar surface, defined by aid of the following considerations:—

In general, when a body emits calorific or luminous radiations, these do not emanate merely from points belonging to the external surface of the body, but also from points situated at certain depths beneath the surface, so that a radiating stratum of a certain thickness has always to be considered. We can, then, legitimately extend to the sun, whatever may be its external constitution, the ordinary definition of a radiating surface. The thickness of the stratum at each point will be defined, as usual, by the distance from the outer surface of the last point whose radiation is sensible beyond that surface. The mean temperature of the radiating stratum (whatever its thickness) at a point will then be named the temperature of the surface at that point; and the true mean temperature of the sun will be the mean of the temperatures of the various points of the surface. We see also what we must understand by the emissive power of the sun at a given point of the surface: it will be the ratio between the intensity of the radiation emitted at that point and the intensity of that which would be emitted by a body possessing an emissive power equal to unity, and raised to the temperature of the sun's surface at the point under consideration; so that the *true temperature of the sun* can also be defined as "the temperature which must be possessed by a body of the same apparent diameter as the sun, in order that, endowed with an emissive power equal to the mean emissive power of the solar surface, it may emit in the same time the same quantity of heat as the sun."

Some experiments made at the Allevard forges with my actinometer, but by the dynamic method, have permitted me to determine the emissive power of steel in fusion, just as it issues, possessing a

temperature of 1500° , from the Martin-Siemens furnace. If we assume that the mean emissive power of the sun is sensibly equal to that of steel in fusion, determined (as I have just said) in the identical conditions of my experiments on the sun*, we arrive at the value of 2000° for the true mean temperature of the solar surface.—*Comptes Rendus de l'Académie des Sciences*, vol. lxxix. pp. 746-749.

PRELIMINARY NOTICE ON A NEW METHOD FOR MEASURING THE
SPECIFIC HEAT OF GASES. BY EILHARD WIEDEMANN.

Since the researches made by M. Regnault, no physicist has resumed in a thorough manner the study of this important question—perhaps on account of the complication and large dimensions of the apparatus to be employed. Now I have succeeded in discovering a method which realizes, with means much more simple, an accuracy as great as that arrived at by M. Regnault with his.

The gas to be studied is enclosed in a balloon of caoutchouc containing about 25 litres kept in an empty balloon of glass by means of a glass tube fixed in a caoutchouc stopper. Another tube serves to put the glass balloon in communication with a second balloon, which communicates with a reservoir full of water placed 10 feet above. A manometer gives the pressure in the interior of the second balloon. When water is brought from the reservoir into this balloon, the air found there is compressed, and the pressure is transmitted to the caoutchouc balloon. A certain quantity of the gas is thus expelled from the caoutchouc balloon into the heating-apparatus and the calorimeter. This quantity can be measured exactly by the weight of the water introduced, account being taken of the temperature and the pressure.

The heating-apparatus consists of a tube 3 metres long and 9 centims. in diameter, completely filled with copper turnings, and placed in a leaden tank full of boiling water. The heating of the gas is complete when 10 litres of it traverse the apparatus per minute.

The calorimeter is composed of a series of tubes of silver 43 millims. in height and 9 millims. wide, filled with silver turnings, and successively traversed by the gas. These silver tubes, three in number, dip into a cylindrical vessel of silvered copper 54 millims. in height and 44 millims. in width, full of water. From the rise of temperature undergone by this water, the equivalent in water of the vessel and the tubes being known, the quantity of heat given up by the gas, and its specific heat, can be deduced.

To avoid radiation, arrangements are made so that the temperature of the surrounding space shall be constant and exactly equal to the mean between the initial and final temperatures of the water

* I have, in fact, verified that, with my actinometer, the dynamic method conducts to exactly the same number for the effective temperature of the sun as the static method.

of the calorimeter. For this purpose the cooling-apparatus is enclosed in a brass box with double sides, the interval between them being filled with water, so as to maintain a very constant temperature all round the apparatus. The double-walled case is moreover protected from the direct radiation of the heating-apparatus by a screen of wood.

The small dimensions of the calorimeter, and the small quantity of water contained by it (about 60 grammes), permit a considerable rise of temperature to be obtained even with a small quantity of gas. Thus, for example, with 20 litres of water, cooled from 100° to about 20° , a rise of 8° of temperature may be observed in the calorimeter.

For the same rise of temperature M. Regnault's method requires about ten times as much, or 200 litres. The diminution of the quantity of gas necessary for the experiment acquires an importance so much the greater as the preparation of the gas presents greater difficulties; and it therefore permits these researches to be extended to a much greater number of gases than they could be with M. Regnault's process.

I have as yet only operated (by way of trial and to control my method) upon air, carbonic acid, hydrogen, and ethylene. For air, first, I obtained, taking at random from a considerable number of measurements made upon this gas:—

0.242
0.236
0.245
0.240
0.237
0.233
0.237
Mean .. 0.237

M. Regnault found 0.2377.

For carbonic acid, I obtained between 25° and 100° :—

0.211
0.208
0.201
0.208
Mean .. 0.208

M. Regnault gives 0.2043.

For hydrogen, my method led to the following results:—

3.398
3.430
3.434
Mean .. 3.431

M. Regnault found 3·409.

For ethylene I obtained

0·3950

0·4070

Mean .. 0·4010

M. Regnault gives 0·4147 and 0·3933, mean 0·4040. In two other experiments, on this last gas adulterated by mixture with a little air, I found 0·3850 and 0·3750—that is to say, a value too low, as was to be expected.

The accordance between these experiments and M. Regnault's is, we see, very satisfactory. I will give, in a subsequent memoir, a more exact and detailed description of the method, with a discussion of the process and the way to eliminate the causes of error. I am at present occupied in extending the experiments to other gases, purposing to study especially the influence of temperature on the specific heat of gases and vapours.—*Bibliothèque Universelle, Archives des Sciences*, vol. li. pp. 73–76.

ON A NEW FORMULA IN DEFINITE INTEGRALS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Will you allow me a few lines to say that the very considerable improvement effected by Mr. O'Kinealy (*Phil. Mag.* for Oct. pp. 295, 296) in the method of proving the formulæ given in the July Number (viz. by replacing the series by a finite expression involving the symbol of operation E) was remarked by me almost as soon as my paper was published, and that, after developing the method so far as to include these formulæ and several others, I communicated it, with the examples, to Professor Cayley, in a letter on the 22nd or 23rd of July, which gave rise to a short correspondence between us on the matter at the end of July. My only reason for wishing to mention this at once is that otherwise, as I hope soon to be able to return to the subject and somewhat develop the principle, which is to a certain extent novel, it might be thought at some future time that I had availed myself of Mr. O'Kinealy's idea without proper acknowledgment.

I remain, Gentlemen,

Yours faithfully,

J. W. L. GLAISHER.

Trinity College, Cambridge,
October 1874.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

DECEMBER 1874.

LVI. *On the Relations between Affinity and the Condensed Symbolic Expressions of Chemical Facts and Changes known as Dissected (Structural) Formulæ.* By C. R. ALDER WRIGHT, D.Sc. (Lond.), Lecturer on Chemistry in St. Mary's Hospital Medical School*.

1. **I**N a paper published in the Philosophical Magazine in April 1872, attention was called to the meanings attached to and the facts summed up in modern chemical formulæ, and to some of the connexions and distinctions between these pithy summaries of knowledge and certain hypotheses assumed by chemists to account for the observed facts. The present essay bears directly on the former and indirectly on the latter of these points, the primary object being to call attention to the necessity which exists for a full and complete study of the amounts of energy involved in chemical changes—a subject of which it may be said that as yet we are almost unacquainted with even the faintest outlines of its scope and bearings.

In the modes of calculation adopted there is little that is new, the author having availed himself largely of the methods of L. Hermann†, Berthelot, Julius Thomsen‡, and others, having simply modified or extended them as occasion served.

2. *Definitions.*—When weights w_1, w_2, w_3, \dots of dissimilar forms of matter A_1, A_2, A_3, \dots coalesce so as to give rise to a weight $w_1 + w_2 + w_3 + \dots = \Sigma(w)$ of a single homogeneous body, this single body is said to be a compound of, or to be composed of,

* Communicated by the Author.

† *Chemisches Centralblatt*, 1869, Nos. 34 & 35.

‡ *Berichte der Deut. Chem. Ges.* vol. v. p. 769.

the forms of matter A_1, A_2, A_3, \dots ; which are also said to *unite* or *combine*, *forming the compound*, or to be *constituents* or *components of the compound*.

These components are said to be united by the *force of chemical affinity*, or, more briefly, *by the force of affinity*.

This force is measured by the work gained during the coalescence. If W absolute units of work are gained during the formation of the weight $\Sigma(w)$ of the compound, the affinity of the constituents A_1, A_2, A_3, \dots in the compound is said to be $\frac{W}{\Sigma(w)}$, or F per unit of weight:—the value of F being positive when W is positive, *i. e.* when *work* is gained during the formation of the compound from its constituents; and negative when W is negative, *i. e.* when *work* is *spent* during the coalescence of the constituents.

For convenience, the absolute unit of affinity (when one absolute work-unit is gained during the formation of a unit of weight of compound) is not employed in the subsequent calculations, a subsidiary affinity-unit being used instead, *viz.* when one calory is produced during the formation of a unit of weight of compound: the relation between the absolute work-unit and the calory is given by the equation

$$W = C \times 0.00024054,$$

or

$$C = W \times 4157.25,$$

where W and C indicate the same amount of work in absolute work-units and calories respectively—the calory being equal to the quantity of heat required to raise the temperature of one gramme of water at maximum density 1 degree Centigrade, and being equal to 423.542 metre-grammes at Manchester, or 4157.25 absolute work-units, g being 9.8155 at Manchester.

Hence the affinity is positive when heat is evolved during the coalescence of the constituents, and negative when heat is absorbed during the process.

3. Conversely, when a weight $\Sigma(w)$ of a homogeneous body can be split up into weights w_1, w_2, w_3, \dots of dissimilar substances, W work-units being spent during the change, the resulting substances are said to be *components* of the compound, and their affinity in the compound is $\frac{W}{\Sigma(w)}$ per unit of weight.

4. A substance is also said to consist of such and such constituents when, although the so-called compound is not directly obtainable by the coalescence of the constituents, nor can it give rise by splitting up to these constituents, it is nevertheless related to these constituents in the same way that veritable com-

pounds and their constituents are related, viz. that, by the action of a weight $\Sigma(w')$ of some other body or series of bodies upon a weight $\Sigma(w)$ of the compound, a series of weights collectively equal to $\Sigma(w + w')$ of new products is formed, precisely the same weights of the same products respectively being formed when the weight $\Sigma(w')$ of these other bodies acts on the weights w_1, w_2, w_3, \dots of the constituents severally.

In these two cases different amounts of work are gained. If the work gained by the action of the weight $\Sigma(w')$ of the reacting bodies on the weight $\Sigma(w)$ of the compound examined be H calories, whilst that gained by the action of the weight $\Sigma(w')$ of the reacting bodies on the weights w_1, w_2, w_3, \dots of the constituents be H' calories, the affinity of the constituents in the compounds is given by the equation

$$F = \frac{H' - H}{\Sigma(w)} \text{ calories per unit of weight of compound.}$$

In this way the affinity of the constituents in a compound may be indirectly determined; in many cases, indeed, this indirect method is the only one practicable.

5. By the conventional use of the terms Combination, Compound, Constituent, Affinity, &c., no question is raised or begged as to whether the constituents are actually *present as such* in the compound: they may be bodily present (as a hypothetical atom of any kind must be bodily present unchanged in qualities in every molecule into which it may be conceived as entering), or they may not (the kind of matter of which the compound is composed being viewed as homogeneous throughout, and differing from that of each and all of its constituents in the same way that these differ from one another); the question is not in any way raised or decided by the language used to express the observed facts.

6. It not unfrequently happens that more than one homogeneous substance is known which is capable of being generated by the coalescence of the same forms of matter, the difference between these *different compounds of the same ingredients* being that the weights w_1, w_2, w_3, \dots of the constituents that go to make up a weight $\Sigma(w)$ of one compound are not all in the same ratios relatively as are the weights w'_1, w'_2, w'_3, \dots of the same constituents respectively that go to make up a weight $\Sigma(w')$ of another of these compounds; *i. e.* the ratios

$$\frac{w_1}{w_2}, \frac{w_2}{w_3}, \frac{w_3}{w_4}, \dots$$

are not all equal to the ratios

$$\frac{w'_1}{w'_2}, \frac{w'_2}{w'_3}, \frac{w'_3}{w'_4}, \dots \text{ severally.}$$

In this case, however, experiment shows that a simple numerical connexion very frequently exists between the values of the ratios of the weights of any given pair of constituents in the two instances. This same connexion is also noticeable when a pair of constituents is common to two compounds, whether the other constituents are severally identical in kind or not. This connexion, known as the *Law of Multiple Proportions*, may be thus stated: If w_1 and w_2 are the weights of any two of the various constituents which jointly form a weight $\Sigma(w)$ of any compound, whilst w'_1 and w'_2 are the weights of the same two constituents respectively which form part or the whole of a weight $\Sigma(w')$ of another compound, then

$$\frac{w_1}{w_2} = \frac{w'_1}{w'_2} \times \frac{m}{n},$$

where m and n are integers comparatively rarely of value exceeding 6 or 8.

This law is not of universal applicability, the whole class of homogeneous substances known as solutions, and various other products obtained by the coalescence of two or more components, being exceptions thereto.

7. It frequently happens that compounds of entirely different properties and characters are produced by the coalescence of the same components in the same relative ratios by weight in every case severally; *i. e.* the ratios $\frac{w_1}{w_2}, \frac{w_2}{w_3}, \frac{w_3}{w_4}, \dots$ are all equal to the ratios $\frac{w'_1}{w'_2}, \frac{w'_2}{w'_3}, \frac{w'_3}{w'_4}, \dots$ severally: in this case the compounds are said to be *isomeric*.

Those isomeric bodies which exhibit a relation in vapour-density such that the vapour-densities of any given pair of isomerides are different simple integral multiples of the same quantity, are said to be *polymeric*.

8. The numerical value of F necessarily varies with the circumstances under which the coalescence of the ingredients takes place, such as temperature, pressure, the physical state of the bodies concerned, &c: if the value of F be known under any given set of conditions, that under any other conditions can be readily calculated when certain thermal properties of the substances concerned are known. Thus, let the affinity between x gramme of a body A, and $1-x$ gramme of another body B, in one gramme of compound be F under certain conditions, say at 0° and 760 millims.; it is required to find what would be the affinity F' under other conditions. Let W_1, W_2 , and W_3 be the total amounts of energy (active, as temperature, electric state, &c.; passive or latent, as undeveloped chemical action, physical state,

&c.) present in x , $1-x$, and 1 gramme of the constituents A and B and the compound respectively; then at 0° and 760 mil-lims.

$$F = W_1 + W_2 - W_3.$$

Let h_1 , h_2 , and h_3 be the number of calories required to alter x gramme of A, $1-x$ gramme of B, and 1 gramme of compound respectively to the new conditions as to pressure, temperature, &c.; then $W_1 + h_1$, $W_2 + h_2$, $W_3 + h_3$ are the total amounts of energy in the two constituents and the compound respectively under the new conditions; therefore

$$F' = (W_1 + h_1) + (W_2 + h_2) - (W_3 + h_3) = F + h_1 + h_2 - h_3.$$

9. Before comparisons can be made of the affinity in classes of compounds exhibiting chemical connexions and relationships, the values of the F 's must be reduced to a common standard. A convenient one for this purpose is that where the affinity is calculated at a uniform temperature such that all bodies concerned are gaseous, and where the materials occupy the same space as the products; it is also convenient to compare together the affinities, not for equal weights of substances, but for such weights as would under constant conditions of temperature and pressure occupy the same space in the gaseous condition. This latter comparison is readily made by multiplying the affinities per gramme of substance (reduced to a common standard) by the vapour-densities of each compound respectively; *i. e.* if F_1 , F_2 , F_3 , ... be the reduced affinities per gramme, and δ_1 , δ_2 , δ_3 , ... the vapour-densities, comparisons are instituted between

$$\delta_1 F_1, \delta_2 F_2, \delta_3 F_3, \dots \&c.$$

In accordance with the conventions (wholly apart from hypotheses as to the constitution of matter) on which chemical symbolic notation is based, the chemical relations in which various substances stand to one another are to a great extent indicated by their respective dissected formulæ. Since each formula indicates a numerical value equal to double the vapour-density, it is more convenient to multiply the affinities per gramme by $2\delta_1$, $2\delta_2$, $2\delta_3$, ... &c. The products $2\delta_1 F_1$, $2\delta_2 F_2$, $2\delta_3 F_3$, ... thus obtained may be termed the *affinities per metrogramme**; and by

* One *metropneum* is the term suggested to indicate the bulk occupied by 1 gramme of hydrogen under the particular circumstances of pressure and temperature that prevail at any given moment; or

$$1 \text{ metropneum} = 11160 \times \frac{273+t}{273} \times \frac{760}{P} \text{ cubic centimetres}$$

at a pressure P and temperature t . The term is used instead of the indefinite phrase "one volume."

A *metrogramme* is the weight in grammes of two metropneums of any

comparing these with the respective dissected formulæ of the substances in question correlations may *à priori* be anticipated.

10. It is proposed thus to compare together the affinities per metrogramme in various bodies all denoted by the general formula $C_m H_n O_p$, *i. e.* all capable of forming carbon dioxide and water and nothing else by the continued action of oxygen, and each one having a vapour-density of $\delta = \frac{12m + n + 16p}{2}$.

It is, however, impossible in this case to compare the affinities at such a uniform temperature that all the bodies concerned are gaseous, carbon being non-volatile at all measurable temperatures. Hence the affinity-values are calculated at some uniform temperature T above or close to the boiling-points of all the bodies concerned, the compound vapours being considered to be under a constant pressure of 760 millims., and the constituents carbon, hydrogen, and oxygen under such a pressure as to occupy jointly the same volume as the compound vapour in each case respectively, the carbon being considered as solid (and hence occupying a space so small as practically to be negligible), and the hydrogen and oxygen gaseous.

100° C. is taken as the constant temperature T , as being practically convenient and sufficiently near to the boiling-points of most of the substances examined to introduce but little error into the calculations from incorrect estimates of latent heat of vapours, &c. To avoid multiplicity of figures, a unit of heat is taken equal to 1000 calories.

11. To calculate the reduced values of $2\delta_1 F_1$, $2\delta_2 F_2$, &c. as thus defined, the following data are employed:—

A. By means of the calorimeter it is found that when one gramme of a given body, $C_m H_n O_p$, is burnt with excess of oxygen so as to form gaseous carbon dioxide and liquid water, H_1 heat-units are evolved, the materials and products being all examined at 15° C. and 760 millims. The values of H_1 are chiefly taken from the experiments of Favre and Silbermann, the results of Dulong, Andrews, and Thomsen being also used, and the mean value being taken when different results are given by different authorities.

B. By means of the calorimeter it is found that, when one gramme of solid carbon is burnt with excess of oxygen to form carbon dioxide, h_1 heat-units are evolved, the materials and products being all examined at 15° and 760 millims.; the value of

given vapour, *i. e.* is the weight indicated by the rational formula of the body in question. The term is suggested as a substitute for "molecule" in cases where no question arises as to theoretical views as to the constitution of matter: *vide* 'Chemical News,' July 18, 1873, p. 25.

h_1 varies with the nature of the carbon employed; charcoal yielding the highest value, this modification is selected, and the mean value 7·964 taken for h_1 :—

Favre and Silbermann	. .	8·080
Despretz	7·912
Andrews	7·900
Mean	. .	7·964

C. One gramme of hydrogen burnt with oxygen to liquid water evolves h_2 heat-units, the materials and products being all examined at 15° and 760 millims.: the mean value 34·275 is taken for h_2 :—

Hess	34·792
Dulong	34·743
Grassi	34·466
Favre and Silbermann	. .	34·462
Julius Thomsen	34·103*
Andrews	33·808
Joule	33·553
Mean	. .	34·275

12. Hence, if $12m$ grammes of charcoal, n grammes of hydrogen, and $16p$ grammes of oxygen were to coalesce, forming 2δ grammes of compound, the temperature being 15° and 760 millims. throughout, the heat evolved would be

$$2\delta F_{15} = 12mh_1 + nh_2 - 2\delta H_1.$$

Now let h_3 be the heat required to raise one gramme of charcoal from 15° to 100°,

h_4 be the heat required to raise one gramme of hydrogen from 15° to 100° under constant pressure,

h_5 be the heat required to raise one gramme of oxygen from 15° to 100° under constant pressure,

and H_2 be the heat required to convert one gramme of compound at 15° into vapour at 100° and 760 millims.;

then the heat generated by the combination of the carbon, hydrogen, and oxygen at 100° to form vapour at 100° would be

$$2\delta F_{100} = 2\delta F_{15} + 12mh_3 + nh_4 + 16ph_5 - 2\delta H_2 = 12m(h_1 + h_3) + n(h_2 + h_4) + 16ph_5 - 2\delta(H_1 + H_2).$$

* *Berichte der Deut. Chem. Ges.* vol. iv. p. 944. The value found for 2·005 grms. of hydrogen uniting with 16 grms. of oxygen to form 18·005 grms. of water is 68·376, whence the value $\frac{68\cdot376}{2\cdot005} = 34\cdot103$ for 1 gram. of hydrogen.

Now

$$h_3 = 0.24 (100 - 15) \times 1.000 = 0.0204,$$

$$h_4 = 3.407 (100 - 15) \times 1.000 = 0.289,$$

and
$$h_5 = 0.218 (100 - 15) \times 1.000 = 0.0185;$$

where 0.24 is the specific heat of carbon between 15° and 100°, and 3.407 and 0.218 the specific heats under constant pressure of hydrogen and oxygen respectively; hence

$$\begin{aligned} 2\delta F_{100} &= 12m (7.964 + 0.0204) + n(34.275 + 0.289) \\ &\quad + 16p \times 0.0185 - 2\delta(H_1 + H_2) \\ &= m \times 95.813 + n \times 34.564 + p \times 0.296 - 2\delta(H_1 + H_2). \end{aligned}$$

13. If, however, the carbon, hydrogen, and oxygen jointly occupied the same volume as the resultant vapour, the heat generated on coalescence at 100° would be less than this amount by a quantity h_6 , representing the work done in compressing the materials from the bulk occupied at 100° and 760 millims. to that occupied by the compound vapour. Neglecting the space occupied by the solid carbon,

$$\begin{aligned} h_6 &= (n + p - 2) \frac{273 + 100}{273} \times 11160 \times \frac{76 \times 13.6}{42,350,000} \\ &= (n + p - 2) \times 0.372, \end{aligned}$$

where $n + p - 2$ is the number of metropneums of contraction taking place when the constituent hydrogen and oxygen are compressed into the same bulk as the resultant vapour.

13.6 is the weight in grammes of one cubic centimetre of mercury, and 42,350,000 is the mechanical equivalent in centimetre-grammes of the unit of heat employed, viz. 1000 calories.

14. Hence, altogether,

$$\begin{aligned} 2\delta F_{100} &= m \times 95.813 + n \times 34.564 + p \times 0.296 - 2\delta(H_1 + H_2) \\ &\quad - (n + p - 2) \times 0.372, \end{aligned}$$

where H_2 is given by the formula

$$H_2 = s_1(t - 15) + l + s_2(100 - t),$$

s_1 being the specific heat of the substance in the liquid state between 15° and its boiling-point, t ,

s_2 being the specific heat, under constant pressure, of the vapour above the boiling-point,

and l the latent heat of vaporization at the boiling-point under a pressure of 760 millims.

When t is above 100, the last term $s_2(100-t)$ becomes negative.

15. By means of this formula the values of $2\delta F_{100}$ for various substances given in the following Table (pp. 410, 411) are calculated. In several instances the data for the calculation of the value of H_2 are only approximately known; and in some the value of H_2 is only estimated as a probable guess. Inasmuch, however, as the experimental errors in the determination of the values of H_1 are considerable, and often as large as the value of H_2 , a small error in the estimation of the latter does not much affect the general result.

The initials affixed to some of the numbers stand for:—

R, Regnault.
K, Kopp.
FS, Favre and Silbermann.
JT, Julius Thomsen.
D, Dulong.
A, Andrews.

16. On examination of the affinity-values given in the Table, it is evident that as any homologous series is ascended there is usually an increase in the value of $2\delta F_{100}$; and so general is this rule, that it seems extremely probable that the few exceptions met with are only apparent exceptions due to experimental errors in the determination of the values of H_1 &c. used in the calculations. Thus the value for ethyl butyrate (147·7) is considerably too high, 110·0 being probably near the true value (§ 39); whilst that for ethyl formate (92·0) is probably also rather too high, as *à priori* the true value may be expected to lie between 68·0 and 91·5, the numbers found for methyl formate and ethyl acetate respectively; probably 86·0 is near the true value (§ 39). Similarly the value for amylic ether (83·5) is probably a little too high, as the substance used by Favre and Silbermann appeared to contain a little amylic alcohol, which would diminish the value found for H_1 , and hence increase the value of $2\delta F_{100}$. On the other hand, the value for stearic acid (154·0) appears to be considerably too low, suggesting the probability of the presence of a little stearine, &c. in the acid used. The values for amyl acetate and methyl, ethyl, and amyl valerates, being deduced from one determination of H_1 only in each case, can only be regarded as approximations. Of these, the values of amyl acetate and methyl valerate are probably the most incorrect, the true values being probably near 114·4 and 110·0 respectively (§ 39): when these corrections are made, and if 105·0 be taken as the value for methyl butyrate instead of 115·1, all the exceptions disappear.

Data for the Calculation of the Values of $2\delta F_{100}$, or the affinity per metrogramme at 100° for various substances. Where two values for $2\delta F_{100}$ are given, the value marked (cor.) is the corrected approximate value as deduced from subsequently described considerations (§§ 16, 29, 34, 39).

Substance.	Formula.	2δ .	s_1 .	s_2 .	l .	t .	H_2 .	H_1 .	$2\delta(H_1 + H_2)$.	h_0 .	$2\delta F_{100}$.	Remarks.
Oxygen	O^2	32	0.218 R	0.0185	0.592	0	0	*Mean value of H_1 (<i>supra</i>).
Hydrogen	H^2	2	3.409 R	0.289	34.275*	69.128	0	0	$H_1 = \begin{cases} 13.108 \text{ A.} \\ 13.063 \text{ FS.} \\ 13.350 \text{ D.} \end{cases}$
Marsh-gas	CH^4	16	0.593 R	0.050	13.174	211.6	0.744	21.7	
Acetylene	$C^2 H^2$	26	0.4 ?	0.03	11.945 JT	311.3	0	-50.6	$H_1 = \begin{cases} 2.431 \text{ A.} \\ 2.490 \text{ D.} \\ 2.403 \text{ FS.} \end{cases}$
Carbon oxide...	CO	28	0.245 R	0.021	2.441	68.9	-0.372	27.5	
Carbon dioxide.	CO^2	44	0.216 R	-80	0.018	0	0.8	0	95.6	$H_1 = \begin{cases} 11.858 \text{ FS.} \\ 11.942 \text{ A.} \\ 11.958 \text{ JT.} \end{cases}$
Ethylene.....	$C^2 H^4$	28	0.404 R	0.034	11.919	334.7	0.744	-5.5	*One estimation of H_1 only.
Amylene.....	$C^5 H^{10}$	70	35	0.1 ?	11.491* FS	811.4	2.976	+10.3	* do.
Diamylene	$C^{10} H^{20}$	140	0.495 FS	60 FS	160	0.1 ?	11.303* FS	1596.4	6.696	46.3	
Cetylene	$C^{16} H^{32}$	224	275	0.1 ?	11.053 FS	2498.7	11.160	129.1	
Tetramylene ..	$C^{20} H^{40}$	280	395	0.1 ?	10.928* FS	3087.8	14.136	196.9	
Water	$H^2 O$	18	1.405 R	536.5 R	100	0.622	0	11.2	0.372	57.9	*One estimation only of H_1 .
Methylalcohol.	$CH^3 O$	32	0.671 FS	0.45 ?	264 $\begin{cases} \text{FS} \\ \text{A} \end{cases}$	65 K	0.313	5.307 FS	179.8	1.116	53.4	$s_1 = 0.645$ between 43° and 23° K.
Ethylic alcohol.	$C^2 H^5 O$	46	0.644 FS	0.4534 R	206	78 K	0.257	6.998	333.7	1.860	63.7	$H_1 = \begin{cases} 6.960 \text{ D.} \\ 6.850 \text{ A.} \\ 7.184 \text{ FS.} \end{cases}$
Amrylic alcohol.	$C^3 H^{12} O$	88	0.587 FS	0.45 ?	121 FS	132 K	0.175	8.959 FS	803.8	4.092	-86.2	$s_1 = 0.564$ between 44° and 26° K.
Cetylic alcohol.	$C^{16} H^{34} O$	242	0.5 ?	0.45 ?	58 FS	350	0.1 ?	10.600 FS	2589.4	12.276	106.7	$\left\{ \begin{array}{l} s_1 = 0.5059 \text{ between fusing- and} \\ \text{boiling-points. Melting-point } 44. \\ \text{Latent heat of fusion } 29.3 \text{ FS.} \end{array} \right.$

Formic acid ...	C	H ²	O ²	46	0.5	0.45 ?	121 FS	105 K	0.162	1.308 JT	67.6	0.744	97.2	One estimation of H ₁ by FS gave 2.091; probably very erroneous. s ₁ = 0.536 between 45° and 24° K. s ₁ = 0.509 between 45° and 24° K. s ₁ = 0.503 between 45° and 21° K.
Acetic acid	C	H ⁴	O ²	60	0.5	0.45 ?	102 FS	117 K	0.145	3.505 FS	219.0	1.488	110.0	
Butyric acid ...	C ⁴	H ⁸	O ²	88	0.5	0.45 ?	115 FS	156 K	0.160	5.647 FS	511.0	2.976	146.3	
Valeric acid ...	C ⁵	H ¹⁰	O ²	102	0.5	0.45 ?	104 FS	176 K	0.150	6.439 FS	672.1	3.720	149.5	
Palmitic acid ...	C ¹⁶	H ³²	O ²	256	0.15 ?	9.3165 FS	2423.4	11.904	204.3	
Stearic acid ...	C ¹⁸	H ³⁶	O ²	284	0.15 ?	9.7165 FS	2802.1	13.392	154.0	Query, pure stearic acid used ? H ₁ = { 9.028 FS. 9.432 D. Not perfectly pure; contained a little amylc alcohol.
Ethylc ether ...	C ⁴	H ¹⁰	O	74	0.503 FS	0.481 R	91 { FS A	35	0.132	9.230	692.8	3.348	33.0	
Amylic ether ...	C ¹⁰	H ²²	O	158	0.5 ?	0.48 ?	69 FS	180	0.113	10.188 FS	1627.6	7.812	83.5	
Methyl formate.	C ²	H ⁴	O ²	60	0.5	0.4 ?	117 A	33 K	0.153	4.197 FS	261.0	1.488	68.0	
Methyl acetate .	C ³	H ⁶	O ²	74	0.5	0.4 ?	110 A	56 K	0.148	5.342 FS	406.3	2.232	87.0	
Methylbutyrate.	C ⁵	H ¹⁰	O ²	102	0.492 FS	0.4 ?	87 FS	95 K	0.128	6.7985 FS	707.5	3.720	{ 115.1 105.0 (cor.)	s ₁ = 0.507 between 41° and 21° K. s ₁ = 0.487 between 45° and 21° K.
Methyl valerate.	C ⁶	H ¹²	O ²	116	0.5	0.4 ?	85 ?	115 K	0.129	7.376* FS	870.6	4.464	{ 115.2 110.0 (cor.)	s ₁ = 0.491 between 45° and 21° K. *Only one estimation of H ₁ .
Ethyl formate...	C ³	H ⁶	O ²	74	0.5	0.4 ?	105 A	55 K	0.143	5.279 FS	401.2	2.232	{ 92.0 86.0 (cor.)	s ₁ = 0.513 between 39° and 20° K.
Ethyl acetate ...	C ⁴	H ⁸	O ²	88	0.483 FS	0.4008 R	99	74 K	0.138	6.293 FS	565.9	2.976	91.5	s ₁ = 0.496 between 45° and 21° K. l = { 106 FS. 93 A.
Ethyl butyrate..	C ⁶	H ¹²	O ²	116	0.5	0.4 ?	90 ?	115 K	0.134	7.091 FS	838.1	4.464	{ 147.7 110.0 (cor.)	*Only one estimation of H ₁ . * do. * do. * do. s ₁ = 0.530 between 41° and 20° K. † Total heat of evaporation at boiling-point = 159° R.
Ethyl valerate...	C ⁷	H ¹⁴	O ²	130	0.5	0.4 ?	85 ?	133 K	0.131	7.835* FS	1035.6	5.208	114.4	
Amyl acetate ...	C ⁷	H ¹⁴	O ²	130	0.5	0.4 ?	85 ?	133 K	0.131	7.971* FS	1053.3	5.208	{ 96.7 114.4 (cor.)	
Amyl valerate...	C ¹⁰	H ²⁰	O ²	172	0.5	0.4 ?	75 ?	188 K	0.126	8.544* FS	1491.2	7.440	151.3	
Cetyl palmitate.	C ³²	H ⁶⁴	O ²	480	0.1 ?	10.342 FS	5012.2	23.808	242.7	
Acetone	C ³	H ⁶	O	58	0.5	0.4125 R	†	56 K	0.169	7.303 FS	433.4	1.860	59.9	†
Phenol	C ⁶	H ⁶	O	94	184	0.15 ?	7.842 FS	751.2	1.860	29.5	

With respect to these corrections, it is evident that the experimental difficulties in the determination of the exact values of H_1 are so considerable that a larger number of determinations than was usually made by Favre and Silbermann is required in each instance before a mean value can be obtained on which perfect reliance can be placed. These experimental errors do not seem materially to affect the nature of a curve drawn, as by Favre and Silbermann, to indicate the rise in value of H_1 as an homologous series is ascended; but they become far more perceptible in the values $2\delta F_{100}$, every such error being there multiplied by 2δ . As shown in § 24, however, a means is afforded of correcting in future these first approximations to the true values of $2\delta F_{100}$ by observing the heat-disturbance occurring during definite chemical reactions under known conditions of temperature, pressure, and physical state.

17. The reduced affinity-values obtained as above afford the means of calculating the amount of heat-disturbance (evolution or absorption) in any given reaction, the materials and products being all supposed to be gaseous, and at the uniform temperature 100° and pressure 760 millims. The heat evolved during such a reaction (H_{100}) must necessarily be given by the equation

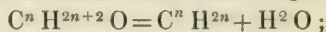
$$H_{100} = \Sigma(F_p) - \Sigma(F_m) + h_7;$$

where $\Sigma(F_p)$ and $\Sigma(F_m)$ are the algebraic sums of the affinity-values of the products and materials respectively, and H_7 the heat-evolution which corresponds to the amount of contraction in volume that takes place during the reaction; for $\Sigma(F_m)$ denotes the heat that would be absorbed were the materials all resolved into their constituent elements, and $\Sigma(F_p)$ the heat developed by the recombination of these elements to form the products, whilst h_7 is the correction for volume-alteration, being equal to

$$m \times 0.372,$$

where m is the number of metropneums (at 100° and 760 millims.) of contraction that take place during the reaction (§ 13).

18. Thus the formation of an olefine and water from an alcohol takes place in accordance with the general equation



for every metrogramme of alcohol -2 metropneums contraction take place during the reaction, or $m = -2$; hence

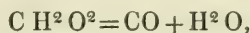
$$\begin{aligned} H_{100} &= \Sigma(F_p) - \Sigma(F_m) - 0.744 \\ &= a + b - c - 0.744; \end{aligned}$$

where a is the affinity-value of the olefine formed, b that of water, and c that of the alcohol used. Thus:—

	a.	b.	c.	Heat-disturbance.
Ethylic alcohol ...	— 5·5	+57·9	63·7	—12·0
Amylic alcohol ...	+ 10·3	57·9	86·2	—18·7
Cetylic alcohol ...	+129·1	57·9	106·7	+79·6

It would hence seem that heat-absorption takes place in the case of lower members of the alcohol series, and heat-evolution in the case of the higher members in this reaction. It may be noted that if the heat-disturbance be taken as $-12\cdot0$ in the case of methylic alcohol, the value of $2\delta F_{100}$ for the as yet unobtained methylene would be $-15\cdot8$, or less than that of ethylene, as might *à priori* be expected, methylene being a lower homologue.

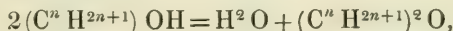
It may also be noticed that the analogous reaction with formic acid,



leads also to heat-absorption, the amount per metrogramme of formic acid being

$$27\cdot5 + 57\cdot9 - 97\cdot2 - 0\cdot744 = -12\cdot5.$$

19. Again, when ethers and steam are generated from alcohol in virtue of the reaction



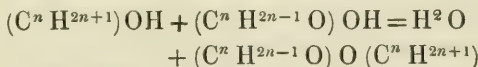
there is *absorption of heat*. In this case $m=0$, and for every two metrogrammes of alcohol the heat-disturbance is given by the equation

$$H_{100} = b + d - 2c;$$

where b is, as before, the affinity-value for water, d that of the resulting ether, and c that of the alcohol employed.

	b.	d.	c.	Heat-disturbance.
Ethylic alcohol ...	+57·9	+33·0	+63·7	—36·5
Amylic alcohol ...	+57·9	+83·5	+86·2	—31·0

20. When acids and alcohols give rise to water and compound ethers by the reactions



heat is uniformly absorbed; here $m=0$, and the heat-disturbance is given by the equation

$$H_{100} = b + e - (c + f);$$

where, as before, b and c denote respectively the affinity-values

for water and the alcohol employed, e that of the resulting ether, and f that of the acid used.

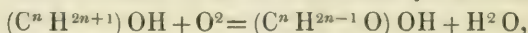
	b .	e .	c .	f .	Heat-disturbance.
Methyl formate ...	57.9	68.0	53.4	97.2	-24.7
" acetate ...	57.9	87.0	53.4	110.0	-18.5
" butyrate...	57.9	115.1	53.4	146.3	-26.7
" valerate ...	57.9	115.2	53.4	149.5	-29.8
Ethyl formate ...	57.9	92.0	63.7	97.2	-11.0
" acetate	57.9	91.5	63.7	110.0	-24.3
" butyrate ...	57.9	147.7	63.7	146.3	- 4.4
" valerate ...	57.9	114.4	63.7	149.5	-40.9
Amyl acetate	57.9	96.7	86.2	110.0	-41.6
" valerate ...	57.9	151.3	86.2	149.5	-26.5
Cetyl palmitate ...	57.9	242.7	106.7	204.3	-10.4

If the corrected values given in the above Table for methyl butyrate and valerate, ethyl formate and butyrate, and amyl acetate be taken instead of the directly calculated numbers, the following heat-disturbances are found:—

	Corrected value of e .	Heat-disturbance.
Methyl butyrate...	105.0	-36.8
Methyl valerate...	110.0	-35.0
Ethyl formate ...	86.0	-17.0
Ethyl butyrate ...	110.0	-42.1
Amyl acetate	114.4	-23.9

so that the heat-absorption is even more plainly visible with the corrected numbers than with those directly calculated.

21. When alcohols are oxidized to acids by the reaction



much heat is developed. Here $m=0$, and the heat-disturbance is given by the equation

$$H_{100} = b + f - (c + g),$$

where b , c , and f indicate, as before, the affinity-values for water, the acid formed, and the alcohol employed respectively, g being the value for oxygen, or zero.

	b .	f .	c .	Heat-disturbance.
Methylic alcohol ..	57.9	97.2	53.4	+101.7
Ethylic alcohol ...	57.9	110.0	63.7	+104.2
Amylic alcohol ...	57.9	149.5	86.2	+121.2
Cetylic alcohol ...	57.9	204.3	106.7	+155.5

22. It may be noticed that the values of H_{100} can also be calculated from the equation

$$H_{100} = \Sigma \{2\delta(H_1 + H_2)\}_m - \Sigma \{2\delta(H_1 + H_2)\}_p,$$

where $\Sigma \{2\delta(H_1 + H_2)\}_m$ and $\Sigma \{2\delta(H_1 + H_2)\}_p$ indicate the algebraic sums of the values of $2\delta(H_1 + H_2)$ for the materials and products respectively; for these quantities indicate respectively the heat that would be generated were the materials in the gaseous state at 100° and 760 millims. burnt to gaseous carbon dioxide and liquid water at 15° and 760 millims., and that absorbed were the resulting carbon dioxide and liquid water at 15° and 760 millims. converted into the gaseous products at 100° and 760 millims. It is easily seen on examination that this equation can be inferred from the previous one involving H_{100} , and the fundamental equation involving $2\delta F_{100}$, from which the numbers in the Table are calculated.

23. If it be required to know the heat-disturbance during any reaction when the materials and products are measured under circumstances different from those above stated (vaporous state at 100° and 760 millims.), this can easily be calculated by means of the formula

$$H_x = H_{100} + \Sigma(h_x)_m - \Sigma(h_x)_p,$$

where $\Sigma(h_x)_m$ and $\Sigma(h_x)_p$ represent the algebraic sums of the amounts of heat absorbed in the alteration of the materials and products respectively to the new conditions. H_x thus becomes

$$= \Sigma \{2\delta(H_1 + H_2) + h_x\}_m - \Sigma \{2\delta(H_1 + H_2) + h_x\}_p.$$

Thus, if ethylic alcohol in the liquid state at 15° give rise to ethylene and liquid water measured at 15° and 760 millims., the value of H_x becomes

$$H_x = \Sigma(2\delta H_1)_m - \Sigma(2\delta H_1)_p,$$

as

$$\Sigma(2\delta H_2)_m = -\Sigma(h_x)_m,$$

and

$$\Sigma(2\delta H_2)_p = -\Sigma(h_x)_p,$$

or

$$H_x = 46 \times 6.998 - (28 \times 11.919 + 18 \times 0) = -11.8.$$

24. By reversing this mode of calculation, it becomes possible to infer from the heat-disturbance taking place in any given reaction under known conditions the heat-disturbance that would take place under the conditions taken as standard (vaporous state at 100° and 760 millims.), and hence to infer the differences between the algebraic sums of $\Sigma(F_p)$ and $\Sigma(F_m)$, or, in other words, to obtain the means of correcting the first approximations

to the true values of $2\delta F_{100}$ obtained by means of the above calculations from the heats of combustion of the substances examined; and further, by this means the values of $2\delta F_{100}$ may be calculated for substances of which the heat of combustion cannot be determined at all, even approximately. The data for such corrections and calculations are as yet extant in only very few instances; but it is to be hoped that before long the study of thermo-chemistry will be further advanced, so that all substances, and not merely a few organic products, may be thus examined. It may perchance be found convenient to alter or modify the standard adopted for the calculations of the values of $2\delta F_{100}$ (vaporous state at 100° and 763 millims., the constituent elements being all at the same temperature, gaseous save in the case of carbon, and occupying jointly the same bulk as the resultant vapour). The object of the present essay is not so much to fix an absolute standard for reduction, as to point out the way for such and to draw attention to some of the results flowing from such calculations.

25. It may here be noticed that the character of some of the values of H_{100} deduced above for various classes of reactions differs from those deduced by Julius Thomsen (*Deut. Chem. Ges. Berichte*, vol. ii. p. 482) from the heats of combustion calculated by Hermann (*ibid.* vol. i. p. 18), on the supposition that the materials and products of combustion are all gaseous at 15° , a correction being made for volume-alterations. Thus, whilst Thomsen recognized that heat is usually (not invariably) absorbed during the formation of a compound ether from an acid and an alcohol, he concluded that the heat-disturbance is imperceptible in the formation of an olefine and water from an alcohol, of an ether and water from an alcohol, and of carbon oxide and water from formic oxide. As regards this latter, however, Thomsen subsequently showed (*Deut. Chem. Ges. Berichte*, vol. v. p. 957) that heat is actually *absorbed* during the decomposition, and is consequently evolved during the synthesis of the acid from carbon oxide and water—this result being arrived at by means of an actual accurate determination of the heat of combustion of formic acid (the one used above, viz. 1.308), and not from a value inferred by analogy from Hermann's reduced value for higher homologous acids; both of which results, however, negative the conclusion drawn by Berthelot from Favre and Silbermann's rough approximation, that heat is *developed* during the decomposition (*Comptes Rendus*, Oct. 10, 1864; *Ann. de Chim. et de Phys.* [4] vol. xviii. p. 24), and render unnecessary in this instance Oppenheim's ingenious speculations as to the cause of such developments of heat (*Bull. Soc. Chim. Paris* [2] vol. ii. p. 419).

26. Some points of considerable interest are revealed by the

comparison of the reduced affinity-values of various substances with their chemical properties as summed up in the dissected formulæ applied to them, and with their physical properties, and notably their boiling-points. The differences between the reduced affinity-values for any given pair of substances is a magnitude representing the heat that would be generated or absorbed by the performance of an "operation"* on the space filled with the vapour of the first substance, whereby the space becomes filled instead with the vapour of the second body, *i. e.* when additional (positive or negative) weight is conferred on the space, and a corresponding alteration made in the properties of the gaseous matter filling it. Thus the operation whereby 2 metropneums of water-vapour at 100° and 760 millims. are converted into 2 of methylic-alcohol vapour under the same conditions is accompanied by a heat-disturbance of $-57.9 + 53.4$, or -4.5 ; and the operation whereby 2 metropneums of marsh-gas are converted into 2 of acetic-acid vapour is accompanied by a heat-disturbance of $-21.7 + 110.0$, or $+88.3$.

Heat-evolution in such cases is not due to the circumstance that more matter is compressed into a given space, as the heat due to this has been allowed for in each case in the correction h_g .

The dissected formulæ ordinarily applied to organic bodies may be taken to indicate to some extent the performance of "operations" whereby one substance may be transformed into another. Thus the formulæ CH^4 and $\text{CH}^3 \cdot \text{CO}^2 \text{H}$ for marsh-gas and acetic acid, or the formulæ $\text{C}^2 \text{H}^5 \cdot \text{OH}$ and $\text{C}^2 \text{H}^5 \cdot \text{O} \cdot \text{C}^2 \text{H}^5$ for ethylic alcohol and ether, represent the hypothetical operations of transformation of 2 metropneums of marsh-gas into 2 of acetic-acid vapour, or of 2 of ethylic alcohol into 2 of ether vapour; and hence certain numerical values (*viz.* the affinity-value differences of the two bodies concerned) are correlative with the differences between two dissected formulæ indicating substances chemically related together.

27. The increase in affinity-value as homologous series are ascended (§ 16) points to the following general rule. Let an operation be performed on a given vapour so as to convert it into a more dense vapour, the chemical relationships between the original and resulting substances being symbolically denoted by the substitution of the group of symbols CH^3 for the symbol H in some constituent hydrocarbonous radical (CH^3 , CH^2 , or CH) in the dissected formula of the original substance; then *heat is evolved during the performance of the operation*, which may for shortness be spoken of as "hydrocarbonous methylation." *A fortiori*, if two or more such operations are performed consecutively (as in

* Brodie, Phil. Trans, 1866.

the operations indicated by the substitution of the radicals C^2H^5 , C^3H^7 , &c. for H), heat is developed.

Thus ethylic, amylic, and cetylic alcohols are derivable from methylic alcohol by operations symbolically indicated by the replacement of an H symbol in the CH^3 group of the formula of methylic alcohol by the radicals CH^3 , C^4H^9 , and $C^{15}H^{31}$ respectively; hence heat is evolved in the transformation of methylic into ethylic alcohol, and also in the conversion of this into amylic alcohol, or of amylic alcohol into cetylic alcohol. Thus:—

Affinity-values.		Heat-disturbance.
Methylic alcohol.. 53·4	Ethylic alcohol ... 63·7	+10·3
Ethylic alcohol ... 63·7	Amylic alcohol ... 86·2	+22·5
Amylic alcohol ... 86·2	Cetylic alcohol ... 106·7	+20·5

It is here noticeable that the conversion of water into methylic alcohol is accompanied by a heat-absorption. But this is not an exception to the above rule, not being a case in point, there being no hydrocarbonous radical in the formula of water; on the other hand, as shown in §§ 31 and 33, this fact is quite in accordance with other general rules. The conversion of hydrogen into marsh-gas, however, is attended with heat-evolution.

28. The following considerations show that *heat is absorbed* when an operation is performed of the kind that may be termed "hydroxylic methylation," *i. e.* symbolically indicated by the substitution of the radical CH^3 for the H symbol in an OH group in the dissected formula of the original substance.

A. The rule holds in the case of acids and their compound methylic ethers, whether the directly calculated affinity-values of the latter or the corrected values be taken.

Acid.	Affinity-value of acid.	Affinity-values of corresponding methylic ethers.		Heat-disturbance.	
		Directly calculated.	Corrected.	Directly calculated.	Corrected.
Formic	97·2	68·0	-29·2
Acetic	110·0	87·0	-23·0
Butyric	146·3	115·1	105·0	-31·2	-41·3
Valeric	149·5	115·2	110·0	-34·3	-39·5

29. B. When an acid is converted into its ethylic ether the process may be regarded as the sum of two operations—one of hydroxylic methylation absorbing heat, and one of the hydrocarbonous methylation evolving heat. Thus the formation of ethyl

acetate from acetic acid may be viewed as made up of the two operations:—

Conversion of $C^2H^3O.OH$ into $C^2H^3O.CH^3$ (hydroxylic methylation).

Conversion of $C^2H^3O.O.CH^3$ into $C^2H^3O.O.CH^2.CH^3$ (hydrocarbonous methylation).

The heat-absorption due to the first operation is considerably greater than the heat-evolution due to the second. Thus the algebraic sum of the two heat-disturbances is negative in the following instances, saving butyric acid and its ether, in which case the irregularity is brought about by the erroneous value found for ethyl butyrate (§ 16). Taking the corrected value, however, a negative algebraic sum is obtained in each case. With ethyl formate a negative value is found even when the uncorrected value is taken.

Acid.	Affinity-value of acid.	Affinity-values of corresponding ethylic ethers.		Heat-disturbance.	
		Directly calculated.	Corrected.	Directly calculated.	Corrected.
Formic ...	97.2	92.0	86.0	-5.2	-11.2
Acetic.....	110.0	91.5	-18.5
Butyric ...	146.3	147.7	110.0	+1.4	-36.3
Valeric ...	149.5	114.4	-35.1

In the case of amylic ethers, it seems that four successive hydrocarbonous methylations only evolve a little more heat than is absorbed during the first hydroxylic methylation.

Acid.	Affinity-value of acid.	Affinity-values of corresponding amylic ethers.		Heat-disturbance.	
		Directly Calculated.	Corrected.	Directly calculated.	Corrected.
Acetic.....	110.0	96.7	114.4	-13.3	+4.4
Valeric ...	149.5	151.3	+1.8

30. C. The rule also holds in the case of alcohols and their simple ethers. The formation of an ether from an alcohol may be viewed as the sum of one hydroxylic and n hydrocarbonous methylations; and in all the known instances of this class the heat-disturbance due to this sum is such as to prove that the heat-absorption during hydroxylic methylation is considerably greater than the heat-evolution during hydrocarbonous methylation.

Affinity-values.		Number of hydrocarbonous methylations for one hydroxylic methylation.	Heat-disturbance.
Ethylic alcohol ... 63·7	Ethylic ether ... 33·0	1	—30·7
Methylic alcohol... 53·4	Ethylic ether ... 33·0	2	—20·4
Amylic alcohol ... 85·2	Amylic ether ... 83·5	4	— 2·7
Ethylic alcohol ... 63·7	Amylic ether ... 83·5	7	+19·8
Methylic alcohol... 53·4	Amylic ether ... 83·5	8	+30·1

These numbers seem to indicate that the heat-absorption in the first hydroxylic methylation is close upon -40 , whilst the heat-evolution for each successive hydrocarbonous methylation is close upon $+10$,—a conclusion nearly the same as that deduced from the acids and amylic ethers, viz. that 4 hydrocarbonous methylations evolve jointly about as much heat as is absorbed during 1 hydroxylic methylation. Manifestly, however, the experimental data used in the calculation of the affinity-values are not known with sufficient accuracy to warrant quantitative deductions of this kind, though they suffice, due allowance being made in two or three instances (*e.g.* ethyl butyrate) for evident large errors, for the deduction of qualitative results, such as the determination of the algebraic sign of the heat-disturbance during a given kind of operation. As above stated (§ 16), the value for amylic ether (83·5) is probably a little too high; the difference, however, will probably not affect the general result.

31. D. The conversion of water into methylic alcohol is a case in point.

Affinity-value.	Affinity-value.	Heat-disturbance.
Water, 57·9	Methylic alcohol, 53·4.	—4·5

It is here noticeable that the heat-absorption in this hydroxylic methylation is considerably less than the heat-evolution during the hydrocarbonous methylation of methylic alcohol producing ethylic alcohol; so that the transformation of water into ethylic alcohol is attended with heat-evolution.

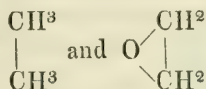
	Heat-disturbance.
Water, 57·9 Ethylic alcohol, 63·7.	+5·8

This result is doubtless connected with the non-existence of carbon as a constituent of water.

32. Another general rule noticeable is, that if an operation be performed symbolically denoted by the replacement of the symbol H^2 by the symbol O , heat is evolved during the process; thus:—

Affinity-values.		Heat-disturbance.		
Marsh-gas	+21.7	Carbon dioxide +95.6	+73.9	
Methylic alcohol,	53.4	Formic acid ...	97.2	+43.8
Ethylic alcohol	63.7	Acetic acid	110.0	+46.3
Amylic alcohol	86.2	Valeric acid ...	149.5	+63.3
Butylic alcohol, between {	63.7 and	Butyric acid ...	146.3	Between { +82.6 and +60.1
Cetylic alcohol	106.7	Palmitic acid ...	204.3	

The substances water and oxygen, however, form an exception to this rule, inasmuch as the transformation of water, H^2O , into oxygen, O^2 , is attended with heat-absorption to the extent of -57.9 ; hence probably the rule of heat-evolution only applies to the substitution of hydrocarbonous H^2 symbols. Unfortunately there are no data for calculating whether such an operation as that symbolized by the formulæ



where the two H symbols replaced belong each to a different hydrocarbonous radical, is attended with heat-evolution or not; but inasmuch as the boiling-point of the oxidized product (ethylene oxide) is above that of the hydrocarbon (ethane), probably heat is evolved during the operation (§ 36).

33. In all the preceding cases the following connexion uniformly holds between the boiling-points and affinity-values of any given pair of substances:—*If an operation be performed on a given substance such that the operation is accompanied by heat-evolution (i. e. if the affinity-value of the product is greater than that of the original substance), the boiling-point of the resulting body (under 760 millims. pressure) is higher than that of the original substance, and vice versa.*

Thus the increase of affinity-value in the ascent of an homologous series is well known to be correlative with an increase in the numerical value of the boiling-point (Kopp). It deserves notice that in the case of water and methylic alcohol and its homologues, where, as shown in §§ 27 and 31, there is heat-absorption in the first stage (hydroxylic methylation), and heat-evolution in each subsequent stage (hydrocarbonous methylation), the boiling-point is *lowered* by the first operation, and *raised* during each successive operation. Thus:—

Substance.	Affinity-value.	Boiling-point.
Water	57.9	100
Methylic alcohol ...	53.4	65
Ethylic alcohol	63.7	78
Amylic alcohol	86.2	132
Cetylic alcohol	106.7	350
	Diff. = - 4.5	Diff. = - 35
	Diff. = +10.3	Diff. = + 13
	Diff. = +22.5	Diff. = + 54
	Diff. = +20.5	Diff. = +218

It should result, therefore, that marsh-gas has a higher boiling-point than hydrogen, *i. e.* will be more readily condensed to a liquid.

34. Similarly, an acid always boils at a higher temperature than the corresponding methylic ether, and, as previously shown (§ 28), has a higher affinity-value. The corrected affinity-values are chosen in the following instances:—

Substance.	Affinity-value.	Boiling-point.
Formic acid	97.2	105
Methyl formate	68.0	33
Acetic acid	110.0	117
Methyl acetate	87.0	56
Butyric acid	146.3	156
Methyl butyrate	105.0	95
Valeric acid	149.5	176
Methyl valerate	110.0	115
	Diff. = -29.2	Diff. = -72
	Diff. = -23.0	Diff. = -61
	Diff. = -41.3	Diff. = -61
	Diff. = -39.5	Diff. = -61

So also with the ethylic æthers:—

Substance.	Affinity-value.	Boiling-point.
Formic acid	97.2	105
Ethyl formate	86.0	55
Acetic acid	110.0	117
Ethyl acetate	91.5	74
Butyric acid	146.3	156
Ethyl butyrate	110.0	115
Valeric acid	149.5	176
Ethyl valerate	114.4	133
	Diff. = -11.2	Diff. = -50
	Diff. = -18.5	Diff. = -43
	Diff. = -36.3	Diff. = -41
	Diff. = -35.1	Diff. = -43

In the case of the amylic ethers the rule also holds, but in the inverse direction, the ether boiling at a higher temperature than the acid, and having a higher affinity-value:—

Substance.	Affinity-value.	Boiling-point.
Acetic acid	110.0	117
Amyl acetate	114.4	133
Valeric acid	149.5	176
Amyl valerate	151.3	188
	Diff. = +4.4	Diff. = +16
	Diff. = +1.8	Diff. = +12

Palmitic acid and cetyl palmitate also accord with the rule in the same way, since the latter has an affinity-value 242·7–204·3, or +38·3 higher than that of the former, whilst its boiling-point is also higher, though how much is unknown.

If the uncorrected affinity-values for ethyl butyrate and amyl acetate be taken, these substances would form apparent exceptions to the above rule, the affinity-value differences being then +1·4 and –13·3 instead of –36·3 and +4·4 respectively. Methyl butyrate and valerate and ethyl formate agree with the rule whichever values be taken.

35. Again, in the case of alcohols and ethers, the rule is observed:—

Substance.	Affinity-value.		Boiling-point.	
Ethylic alcohol	63·7	} Diff. = –30·7	78	} Diff. = – 43
Ethylic ether.....	33·0		35	
Methylic alcohol	53·4	} Diff. = –20·4	65	} Diff. = – 30
Ethylic ether	33·0		35	
Amylic alcohol	86·2	} Diff. = – 2·5	132	} Diff. = + 48
Amylic ether	83·5		180	
Ethylic alcohol	63·7	} Diff. = +19·8	78	} Diff. = +102
Amylic ether	83·5		180	
Methylic alcohol	53·4	} Diff. = +30·1	65	} Diff. = +115
Amylic ether	83·5		180	

There is one apparent exception to the generality of the rule, viz. amylic alcohol and ether. Concerning this case and certain others where the rule only holds with some limitation, *vide* § 37; it is, however, clearly noticeable that the *direction* of the boiling-point alterations is all the way through the series the same as that of the affinity-value differences; *i. e.* in each series each difference is obtainable from the previous one by the addition thereto of a positive quantity.

36. The rule holds in cases of substitution of H² by O; and it is noticeable that in the case of the substances water and oxygen, where the affinity-value is *lowered* by this operation, the boiling-point is *also lowered*; so that this apparent exception is, like the case of water and methylic alcohol, only a particular case of a more general rule.

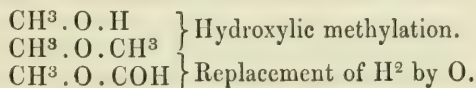
Substance.	Affinity-value.		Boiling-point.	
Water	+57·9	Diff. = -57·9	100	Diff. = -300?
Oxygen	0		-200?	
Marsh-gas	+21·7	Diff. = +73·9	-200?	Diff. = +120?
Carbon dioxide	+95·6		- 80	
Methylic alcohol	53·4	Diff. = +43·8	65	Diff. = + 40
Formic acid	97·2		105	
Ethylic alcohol	63·7	Diff. = +46·3	78	Diff. = + 39
Acetic acid	110·0		117	
Amylic alcohol	86·2	Diff. = +63·3	132	Diff. = + 44
Valeric acid	149·5		176	
Cetylic alcohol	106·7	Diff. = +97·6	350	Diff. positive.
Palmitic acid	204·3		Above 350	

In connexion with this, it may be noticed that aldehydes and ketones usually boil at higher temperatures than the hydrocarbons from the formulæ of which their formulæ are deducible by replacing H^2 by O —*e. g.* dimethyl and ethylic aldehyde, diamylene and borneol, cymene and cuminic aldehyde, propane and acetone, heptane and butyrone, ethylbenzene and methylphenyl-ketone, &c. Hence it may be inferred that, when the requisite data are extant for calculating the affinity-values of these classes of substances, it will be found that the value for an aldehyde or ketone is higher than that for the corresponding hydrocarbon; and probably the same remark applies to bodies represented by formulæ of character analogous to that of ethylene oxide (§ 32). Thus it will probably be found that the affinity-value for propane is less than 59·9, the value obtained for acetone; whilst it must be above 21·7, the value found for marsh-gas.

37. The rule enunciated above, connecting the change in boiling-point and in affinity-value of a body by the performance of a given operation, although holding in every case where *only a single operation* is performed (*e. g.* hydrocarbonous, or hydroxylic methylation, or substitution of H^2 by O), and also holding in many cases where a *succession of operations* is performed, is nevertheless not invariably observed in cases when several operations are consecutively performed, some of which would tend to cause an alteration in one direction and some in another. And in point of fact such could not fail to be the case, since the alterations in boiling-point are not always directly *proportionate* to the alterations in affinity-value, but only *vary in the same direction with them*; hence the algebraic sum of a series of alterations in affinity-value produced by a succession of operations may readily be +, whilst the algebraic sum of the corresponding alterations in boiling-point may be —, and *vice versa*. A case

in point is afforded by amylic alcohol and amylic ether (§ 35), where the result of one hydroxylic and four hydrocarbonous methylations gives a positive increase in boiling-point, but a negative one in affinity-value.

Similarly, when certain alcohols and their compound ethers are compared, the same kind of result follows. Thus with methylic alcohol and methyl formate; the latter may be regarded as derived from the former by the sum of two successive operations, viz. :—



The first gives rise to heat-absorption and corresponding lowering of boiling-point, the second to heat-evolution and rise of boiling-point; but the algebraic sum of the changes in affinity-value is +, and that of boiling-point —.

Substance.	Affinity-value.	Boiling-point.
Methylic alcohol	53·4	65
Methylic formate;	68·0	33
	Diff. = +14·6	Diff. = —32

38. The phenomena of isomerism and polymerism appear also to be subject to the influence of the above thermal rule, a polymeride or isomeride of higher boiling-point having uniformly a higher affinity-value than one of lower boiling-point; *i. e.* if an operation be performed upon a given substance such that it becomes converted into an isomeric or polymeric body of higher boiling-point, heat is evolved during the operation, and *vice versâ*.

Thus :—

Substance.	Affinity-value.	Boiling-point.
Acetone	59·9	56
Methyl valerate	110·0	115
Ethylene	— 5·5	—100?
Amylene	+ 10·3	+ 35
Diamylene	+ 46·3	+160
Cetylene	+129·1	+275
Tetramylene	+196·9	+395
	Diff. = +50·1	Diff. = + 59
	Diff. = +15·8	Diff. = +135?
	Diff. = +36·0	Diff. = +125
	Diff. = +82·8	Diff. = +115
	Diff. = +67·8	Diff. = +120
Methyl formate	63·0	33
Acetic acid	110·0	117
Ethyl acetate	91·5	74
Butyric acid	146·3	156
Methyl butyrate ...	105·0	95
Valeric acid	149·5	176
	Diff. = +42·0	Diff. = + 84
	Diff. +54·8	Diff. = + 82
	Diff. +44·5	Diff. = + 81

If the uncorrected value for methyl butyrate and valerate be

taken, the affinity-value differences become +24.4 and +55.3 instead of +44.5 and +50.1 respectively, the algebraic sign remaining unchanged in each case.

The fact of evolution of heat during the process of polymerizing aldehydes, olefines, terpenes, &c. is very well known in numerous instances, although the exact heat-development has not been accurately measured in any particular case. Kekulé has recently shown (*Berichte Deut. Chem. Ges.* vol. vi. p. 1087) that when carvol is converted into its isomeride carvacrol, boiling some 12° higher, much heat is developed during the reaction.

It may hence be predicted that the affinity-value of a normal acid, alcohol, hydrocarbon, &c. will be found to be higher than that of the corresponding secondary or tertiary isomeride, since the boiling-point usually appears to be higher; for instance, normal propylic alcohol will have a higher value than isopropylic alcohol.

39. The following three cases afford apparent exceptions to the generality of the preceding rule as to isomerides when the uncorrected affinity-values are taken for calculation:—

Substance.	Affinity-value.	Boiling-point.
Methyl acetate	87.0	56
Ethyl formate	92.0	55
Methyl valerate	115.2	115
Ethyl butyrate	144.7	115
Ethyl valerate	114.4	133
Amyl acetate	96.7	133
	Diff. = + 5.0	Diff. = - 1
	Diff. = +29.5	Diff. = 0
	Diff. = -17.7	Diff. = 0

If, however, the corrected values be taken instead, these apparent exceptions disappear.

Substance.	Affinity-value.	Boiling-point.
Methyl acetate	87.0	56
Ethyl formate	86.0	55
Methyl valerate	110.0	115
Ethyl butyrate	110.0	115
Ethyl valerate	114.4	133
Amyl acetate	114.4	133
	Diff. = -1.0	Diff. = -1
	Diff. = 0	Diff. = 0
	Diff. = 0	Diff. = 0

It may here be noticed that if the corrected values given in the Table for methyl butyrate and valerate, ethyl formate and butyrate, and amyl acetate be taken, these substances agree perfectly with the general rules elucidated in §§ 16, 29, 34, and 39; whereas if the directly calculated values be chosen, these bodies form exceptions to most or all of the rules; and as there is not the slightest improbability in the character or amount of the cor-

rections, it may fairly be concluded that the directly calculated values are erroneous to the extent thereby indicated.

The wonder is, not that a few of the affinity-values calculated as above from data often defective or approximative only should be clearly somewhat erroneous, but that so large a number of instances should be found indicating that the reduced affinity-values thus obtained are not far removed from the actual true values, and that it should be possible from experimental determinations so incomplete to deduce general rules of so wide an application and subject to so few apparent exceptions.

40. If Favre and Silbermann's determinations of the heats of combustion of various terpenes could be thought to be absolutely exact, these substances would form a well-marked exception to the thermal rule above stated.

	Boiling-point.	Value of H_1 .	Affinity-value.
Terebene	156	10·662	A.
Oil of turpentine ...	161	10·852	Less than A.
Oil of lemons,	173	10·958	Still smaller.

So that rise in boiling-point would be accompanied by diminution of affinity-value. But it has been shown by the writer, and also by Orlowski, that distilled oil of turpentine naturally contains cymene; the same remark probably also applies to oil of lemons; whilst Riban has shown that the process ordinarily used for preparing terebene from turpentine-oil (by means of sulphuric acid) gives a product containing much cymene. Hence in all probability the bodies examined by Favre and Silbermann were not pure homogeneous liquids; and therefore no conclusion can be drawn from the difference in their heats of combustion.

41. In addition to the foregoing conclusions, some others may be drawn from the affinity-values calculated above. Thus:—

When two substances have formulæ differing only by C symbols, *the one with the higher number of C symbols has a less affinity-value than the other.*

Affinity-values.		Number of additional C symbols.	Difference of affinity-values.
Marsh-gas ... 21·7	Ethylene ... — 5·5	1	—27·2
Alcohol 63·7	Acetone ... +59·9	1	— 3·8
Acetone 59·9	Phenol..... +29·5	3	—30·4
Alcohol 63·7	Phenol..... +29·5	4	—34·2

When the formulæ differ only by H symbols, it would seem that *the one with more H symbols has a higher affinity-value than the other.*

Affinity-values.		Number of additional H symbols.	Difference of affinity-values.
Acetylene -50.6	Ethylene -5.5	2	+45.1
Acetone +59.9	{ Isopropylic alcohol between +63.7 and +86.2 }	2	Between +3.8 and +26.3
Carbon dioxide.. 95.6	Formic acid ... 97.2	2	+ 1.6

When they differ only in O symbols, the one with more O symbols has a higher affinity-value than the other.

Affinity-values.			Number of additional O symbols.	Difference of affinity-values.	
Hydrogen...	0	Water	57.9	1	+ 57.9
Marsh-gas...	21.7	Methylic alcohol	53.4	1	+ 31.7
Ethylene ...	- 5.5	Acetic acid	110.0	2	+ 115.5
Amylene ...	+10.3	Valeric acid.....	149.3	2	+ 139.2
Acetone	59.9	Methyl acetate..	87.0	1	+ 27.1

It is manifest, however, that but little stress can be laid on these conclusions. As the operation termed "methylation" produces opposite effects according as it is hydroxylic or hydrocarbonous, and as the same probably holds in the case of the operation indicated by the replacement of H^2 by O, it is clear that the addition of a C, H, or O symbol to one part of a formula or to another may represent a different operation in each case respectively, and hence may not always correspond to the same kind of heat-disturbance.

43. The foregoing calculations lead to the conclusion that the different positions with respect to one another occupied by the various constituent radicals in a dissected formula indicate not only differences in chemical reactions and physical properties, but also correlative differences in the amounts of work performed or gained in the formation (under constant conditions) of the substances thereby indicated from those forms of matter conventionally termed their constituent elements, and hence indicate differences in the amount of what may be conveniently termed the *Intrinsic Chemical Energy* possessed by the substances in question, *i. e.* the energy present in forms other than those due to physical state, temperature, or states of electrification, magnetization, mechanical motion, &c.

The atomic hypothesis being admitted (that matter is made up of indestructible indivisible fragments of matter of at least some sixty-five essentially different kinds, which when associated together into groups form molecules of various kinds, every homogeneous body being made up of a large number of such mo-

lecules, each one identical with all the other molecules of the same body, but different from every other kind of molecule), it results that an alteration in the total intrinsic chemical energy present in a given quantity of matter, consisting of one or more homogeneous bodies, whereby a different body or set of bodies is produced, can only be brought about by an alteration in the nature or amount of the relative motions either of the molecules or of their constituent atoms, or of both. Whatever may be the character of the motions before and after the change, they must take place under the influence of forces regulated by definite laws, the law of force regulating the action of one atom or molecule on another not being necessarily independent of the nature of the atoms or molecules: *e. g.* the law of force between two hydrogen atoms is not necessarily the same as that between a hydrogen and an oxygen or a hydrogen and a chlorine atom; nor is that between two molecules of water necessarily the same as that between a molecule of water and one of hydrochloric acid, or between one of water and one of chloride of sodium. Whatever the nature of these laws of force, however, they must be such that the single resultant force between any two masses of matter, at a distance from one another great as regards the size of the molecules that make up the masses, is inversely proportional to the square of the distance, and directly proportional to the product of the masses (Newton), and such that the single resultant forces between the constituent molecules themselves are compatible with the phenomena of change of physical state, elasticity, viscosity, cohesion, expansion, diffusion, &c.

The study of the energy-differences brought about in chemical actions affords a probable means of arriving at the laws which must regulate the mutual action of these hypothetical ultimate atoms of different kinds on one another, and hence of deducing the resultant forces between molecules of various kinds, and consequently of accounting on mechanical grounds for all chemical and physical phenomena. Should it be hereafter found in this way that such laws of force are possible, a great addition to our knowledge will accrue, and the hypothesis of the atomic constitution of matter will be demonstrated with as much certainty as it is possible that any physical "law" can be demonstrated. On the other hand, should it be clearly proved that the law of force between any given pair of atoms deducible from one set of phenomena is incompatible with that obtained from another set, or that the interatomic forces are irreconcilable with the physical intermolecular resultant forces deducible from the study of physical phenomena, or with gravitation, or that any other incompatibilities ensue, it must follow that the primary hypothesis of the atomic constitution of matter is untenable.

LVII. *The Hydrodynamical Theory of the Action of a Galvanic Coil on an external small Magnet.*—Part III. By Professor CHALLIS, M.A., F.R.S.

[Continued from p. 363.]

TO complete the arguments contained in Parts I. and II. relative to the action of a galvanic coil, it remains to employ the hydrodynamical theory of galvanism to account for the experimental basis of the explanation of that action given by Ampère's theory. The facts and hypotheses that will be referred to for this purpose are for the most part contained in *Leçons 70 and 71* of Jamin's *Cours de Physique*, vol. iii., and for the sake of convenience will be adduced by citation from that work. The articles in this third Part are numbered in continuation of those of Part II.

55. Two parallel rheophores are mutually attracted or repelled according as the currents along them are in the same or opposite directions (see Jamin, p. 198). Before proceeding to give a theoretical demonstration of this law, a few preliminary remarks are required.

It may be first stated that since according to one of the fundamental hypotheses enunciated in art. 1 "the size of the atoms is supposed to be so small that even in dense bodies they fill a very small portion of a given space," it will be assumed that the steady motions that will come under consideration are not sensibly modified by the mere occupation of space by the atoms. Again, it will be admitted, as before in art. 19, that in steady motion the accelerative action of the fluid on an atom has a constant ratio to the accelerative force of the fluid itself at the position of the atom, and is in the same direction. Hence, since in the case of the parallel rheophores the mutual action may be assumed to take place in directions transverse to the axes, if p be the pressure at any position distant by r from a fixed line of reference parallel to the axes and in the same plane, the accelerative action on an atom in that position will be proportional to $\frac{dp}{dr}$. It is further to be remarked that the law arrived at in art.

35, according to which the velocity parallel to the axis of a straight rheophore varies inversely as the distance from the axis, does not apply to positions within its cylindrical surface, because otherwise the velocity would be infinite along the axis. Also, for a like reason, there cannot be within that surface transverse motion varying as some inverse power of the distance from the axis. Hence it is necessary to conclude that at distances from the axis less than the radius of the rheophore the

motion is such that $udx + vdy + wdz$ is integrable of itself and not by a factor, and consequently that at all such distances the velocity parallel to the axis is the same. If $V_1 =$ that velocity in the interior and at the surface and $\beta =$ the radius of the rheophore, the velocity at any distance r from the axis greater than β may be assumed to be $\frac{V_1\beta}{r}$. Hence, since β is in general very small, in order that the exterior velocity parallel to the axis may be of considerable magnitude, it is necessary that V_1 should be large.

56. These conclusions being admitted, if at the position of any atom of the rheophore within which the velocity parallel to the axis is V_1 , the longitudinal velocity due to the other rheophore be v_1 , and the transverse velocity due to the same be v_2 , we shall have by hydrodynamics

$$p = C - \frac{1}{2}((V_1 + v_1)^2 + v_2^2).$$

Hence, if r be the distance of the atom from the fixed line of reference mentioned in the preceding article, we shall have, V_1 being constant,

$$\begin{aligned} \frac{dp}{dr} &= -(V_1 + v_1) \frac{dv_1}{dr} - v_2 \frac{dv_2}{dr} \\ &= -V_1 \left(\left(1 + \frac{v_1}{V_1}\right) \frac{dv_1}{dr} + \frac{v_2}{V_1} \frac{dv_2}{dr} \right). \end{aligned}$$

Consequently, since from the foregoing argument the ratio $\frac{v_1}{V_1}$ is very small if the distance between the rheophores be a large multiple of the radius of that to which the velocity v_1 pertains, and since *à fortiori* this is the case with respect to the ratio $\frac{v_2}{V_1}$ because v_2 varies inversely as the square of that distance, it follows that, under the circumstances in which the experiments to which the argument applies were made, we have very approximately

$$\frac{dp}{dr} = -V_1 \frac{dv_1}{dr}.$$

If the distance of the axis of the nearer rheophore from the reference line be h , we may put $\frac{V'_1\beta}{r-h}$ for v_1 ; and we thus get

$$\frac{dp}{dr} = \frac{V_1 V'_1 \beta}{(r-h)^2}.$$

It thus appears that if V_1 and V'_1 have the same sign, or the

currents be in the *same* direction, $\frac{dp}{dr}$ is a positive quantity, and p increases with r , so that the action tends to diminish the interval $r-h$, or is *attractive*. But if V_1 and V'_1 have *different* signs, or the currents be in *contrary* directions, $\frac{dp}{dr}$ is *negative*, and the action is *repulsive*. It is plain that the argument applies to the action of either rheophore on the other.

57. Since in the foregoing reasoning the *lengths* of the rheophores were not involved, we may draw from it the important corollary, that, if two *elements* of parallel currents be so situated that the straight line joining their middle points is perpendicular to their axes, the mutual action between the elements *varies inversely as the square of the interval between the axes*. It is to be noticed that this law has thus been independently derived by argument from the fundamental hypotheses.

58. "We may always replace a rectilinear current by any *sinuous* current in the same general direction, provided it is very little distant from the first" (Jamin, p. 200). This is another of the facts, or laws, which, as forming the basis of Ampère's theory, it is proposed to account for by the hydrodynamical theory. The following is, I think, the appropriate argument in this instance.

According to the principles adduced in arts. 11 and 34, in order that any element of a galvanic current may transfer a portion of fluid across a plane perpendicular to its direction, it is necessary that there should be a *stress*, of the nature of hydrostatic pressure, to counteract the resistance arising from the inertia of the surrounding fluid, and that the total current of which the element is a part should *circulate* by means of a rheophore. The first condition is fulfilled by the force which has its origin at the galvanic battery, and the other by a closed conductor. Now this stress, like any hydrostatical force, is resolvable into components in rectangular directions, and in the instance of the sinuous rheophore may be resolved at each point in the *mean* direction of the rheophore, and in two directions at right angles to this. The mode of action of the stress may be conceived of by reference to the proof of the equality of hydrostatic pressure in all directions usually given in elementary treatises on Hydrostatics. A closed vessel of any form is supposed to be completely filled with incompressible fluid; and by means of a piston inserted into a side of the vessel a pressure is produced which by the intervention of the fluid is equally exerted at all points of its interior and of the enveloping surface, so that if at any other part of the containing vessel a movable piston were inserted, it would be put in motion by the

pressure. In an analogous manner the stress necessary for the maintenance of the motion in any element of a galvanic current is capable of operating in all directions, but produces no motion unless the means of *circulating* be provided. Such provision is made when another complete current is placed in the neighbourhood of the first; and on this principle mutual action, depending on the coexistence of the motions, may take place between each element of one rheophore and all the elements of the others. These views being adopted, the motions of either of two rheophores resulting from this composite action of their currents may be determined by the following considerations. [I take occasion to remark that in 'The Principles of Physics,' p. 601, galvanic *induction* is attributed to the sudden action of a state of stress like that supposed above.]

59. First, as was remarked in art. 58, the above-mentioned stress admits, for the same reason as hydrostatic pressure, of being resolved in rectangular directions; but inasmuch as it is so intimately related to a galvanic current that the one does not exist without the other, the resolution must take place *within* the compass of each element of the rheophore, and so as to give rise to *elementary* galvanic currents of the ascertained type. It is next to be considered that, according to the reasoning contained in arts. 55 and 56, the action of one rheophore tends to produce motion of translation of the other only when a current pertaining to the former flows within the superficial boundary of the other in a direction parallel to its axis, and varies from point to point in the directions perpendicular to the axis in such manner that the resulting accelerative force acts in the same directions. Reserving at present a more general application of these principles, I propose now to apply them in the solution of the problem of the action of a *sinuous* current.

60. Taking the experiment, at first, as made by Ampère, the sinuous rheophore and the straight return one in the mean direction of the other will be supposed to be rigidly connected and fixed, and to act upon a movable one parallel to the latter. If, in conformity with the principles above laid down, each element of the sinuous current be resolved in directions parallel and transverse to the mean direction, the sum of the latter resolved parts will have no translatory effect on the movable rheophore, because they are wholly in directions transverse to its axis. The other resolved parts will tend to move the rheophore in the manner described in arts. 55 and 56; and as their sum is just equal to the return current but opposite in direction, they will have the effect of neutralizing the translatory action of that current, so that no motion of the movable rheophore will ensue.

In Jamin (p. 201) the sinuous and return currents are represented by a figure as being connected, but movable about an axis, and the other rheophore as fixed. In this case let the velocity W within the sinuous rheophore and parallel to its axis, be resolved at any point into $W \cos \alpha$ parallel to the current's *mean* direction, and $W \sin \alpha$ in the plane through the point transverse to that direction, and let w be the velocity at the same point due to the current of the fixed rheophore. Then the velocity $W \sin \alpha$, as being transverse to the axis of motion, will have no effect; but the composition of $W \cos \alpha$ and w will give rise to a translatory action on the element of the sinuous rheophore which, by the same reasoning as that in arts. 55 and 56, may be expressed by

$$-W \cos \alpha \frac{dw}{dr} ds,$$

ds being the length of the element, and r the distance of the given point from the axis of the fixed rheophore. Now, as W is the same for the sinuous current as for the return straight one, and $ds \cos \alpha$ is the projection of the element ds on the latter, it follows that the sum of all the elementary actions is the same for both rheophores, but in opposite directions, and that consequently, as is found by experiment, no motion takes place.

61. We may now proceed to obtain a general formula for the action of an element of one rheophore upon an element of another, as due to the hydrodynamical action of the corresponding elements of the currents pertaining to the two rheophores. Let ds be the length of the element of one of the rheophores, and ds' the length of the element of the other; and, supposing their axes to have any relative positions, let their middle points be joined by a straight line of length r . Also let θ and θ' be respectively the inclinations of the axes to the joining line, and conceive the plane containing the element ds' and that line to be inclined by the angle ϵ to the plane containing ds and the same line. Hence, V and V' being the velocities of the currents at the middle points of the elements, the resolved parts along the joining line are $V \cos \theta$ and $V' \cos \theta'$, and those perpendicular to the same are $V \sin \theta$ and $V' \sin \theta'$. The resolved part of the last of these velocities in the plane containing the velocity $V \sin \theta$ is $V' \sin \theta' \cos \epsilon$; so that the velocities $V \sin \theta$ and $V' \sin \theta' \cos \epsilon$ are *parallel* to each other and perpendicular to the line of junction. The resolved part perpendicular to the same plane is $V' \sin \epsilon$.

62. It is evident that the velocities V and V' may be taken to represent the *intrinsic intensities* of the two currents. Hence $V ds$ and $V' ds'$ will be respectively proportional to the products of

the intensities and lengths of the original elements of currents ; and accordingly $V \sin \theta ds$ and $V \cos \theta ds$ will be like products for the resolved parts of one of the original elements, and $V' \sin \theta' \cos \epsilon ds'$, $V' \cos \theta' ds'$, and $V' \sin \epsilon ds'$ those for the resolved parts of the other. It is important to remark that these several products represent elementary *moments* of currents, and that the stresses required for maintaining these moments are, in fact, those to which has hitherto been ascribed the generation of the motions of the æther by the hydrodynamical operation of which an element of one rheophore acts upon an element of another. Now, according to what is said in art. 59, the original elementary currents and their resolved parts must all be supposed to partake of the characteristic type of a galvanic current ; and hence it may be shown, by arguing just as in arts. 55 and 56, that an element of a rheophore acts upon an element of another rheophore only by means of those motions due to the resolved parts of the respective elementary currents which within the limits of the element acted upon are *parallel* to each other. Hence the velocities $V \cos \theta$, $V' \cos \theta'$, and $V' \sin \epsilon$ are excluded from consideration. Respecting the exclusion of the first two more will be said subsequently.

63. From these preliminaries we may proceed to obtain a formula for the action of either of the elements on the other. It will, at first, be supposed that the element whose action is investigated is that to which the velocity V belongs. At the distance r from the middle of this element, in the direction of the line joining the middle points of the two elements, the velocity transverse to that line may, according to the law obtained in art. 43 and the argument of art. 62, be expressed as $\frac{\mu V}{r} \sin \theta$, μ being a constant factor depending on the dimensions and atomic constitution of the element whose action the formula is intended to express. The parallel velocity within the other element is $V' \sin \theta' \cos \epsilon$; and as the action on the latter element will *ceteris paribus* be proportional to the number of its atoms, and that number is in some fixed proportion to ds' , the expression required may be supposed to contain the factor $\mu' ds'$, μ' being a constant dependent on the constitution of the element acted upon. It must also contain ds as a factor, because the action of the elementary current is in a certain proportion not simply to the velocity V , but, as explained in art. 62, to the *moment* represented by Vds . Hence, by reference to the formula for $\frac{dp}{dr}$ obtained in art. 56; it will be seen that the complete expression required, inasmuch as the total action has a constant ratio to

the sum of the values $\frac{dp}{dr}$ for the positions of all the atoms of the element acted upon, is composed of the factors $V' \sin \theta' \cos \epsilon$, $\mu' ds'$, and the differential coefficient of $-\frac{\mu V}{r} ds \sin \theta$ with respect to r , together with a constant factor F_0 . Hence, putting F for the value of the expression, we have

$$\frac{F}{F_0} = \frac{\mu \mu' V V'}{r^2} \sin \theta \sin \theta' \cos \epsilon ds ds'. \quad . \quad . \quad . \quad (c)$$

It is plain from the form of this formula that by a change of the constant F_0 it would express the action of the element whose intrinsic intensity is V' on that whose intrinsic intensity is V , although investigated only with reference to the action of the latter.

64. The above expression for $\frac{F}{F_0}$ is identical with the *first* term of the well-known formula obtained by Ampère for the mutual action between two galvanic elements. That formula, as given in Jamin, p. 207, contains an additional term, involving a constant factor k , the numerical value of which is subsequently determined by means of a certain analytical process. The reason assigned for adding this term, and the analysis employed for determining the value of k , rest upon two experimental facts, which, as I propose in the next place to show, admit of being accounted for by the hydrodynamical theory.

65. The first experiment, made originally by Ampère, is that represented by a figure in Jamin, p. 200, according to which a current entering at one end, and issuing at the other, of a bent movable rheophore the terminals of which point towards the same quarter in parallel directions, is caused to move in the direction of the entering of the current. In 'The Principles of Physics,' p. 591, I have given an explanation on hydrodynamical principles of a very similar experiment made by Faraday. According to this explanation, which is equally applicable to the present instance, the current enters the rheophore convergently and with increasing velocity, and consequently, by the hydrodynamics of steady motion, with *diminishing* pressure, in the direction of entering, whilst it issues divergently with diminishing velocity, and *increasing* pressure, in the direction of issuing; so that within certain portions of the rheophore at both ends the decrements of pressure are in the *same* direction, and have consequently the effect of a *repulsive* action in that direction.

66. It is evident, however, that this result depends on the rheophore being *bent*. In the case of a straight rheophore, or

one slightly curved, the hydrodynamical pressures in directions along the axis and at the ends would neutralize each other, and no motion would result from the passage of a galvanic current through it. This theoretical inference is proved to be true by means of the second of the two experiments referred to above, the method of performing which, as originally arranged by Ampère, is exhibited by figure 564 in p. 207 of Jamin's work.

67. It thus appears that both experiments are explainable by the hydrodynamical theory; and hence, if Ampère has legitimately deduced from them the second term of the expression for F , and the numerical value of the constant k , these results may be claimed as properly belonging to that theory. I must, however, say that I do not see how the two experiments taken together justify the supposition of a repulsive force between elements having their axes in the same straight line; they rather seem to me to point to the conclusion that the action between elements under these circumstances does not come into account as respects the production of motion of translation, because the resulting forces at the extremities of a finite rheophore are equal in opposite directions, and those between elements in juxtaposition are neutralized. On the principles of the hydrodynamical theory it may be argued that the parts $V \cos \theta$ and $V' \cos \theta'$ of the original elementary currents, resolved, as stated in art. 61, along the line joining their middle points, give rise to no action between the elements of the rheophores, because the motions which, according to the principles of the resolution of galvanic currents explained in arts. 59 and 62, belong to these resolved parts are incapable of producing by composition variations of pressure at the positions of the atoms of either of the elements of the rheophores.

68. The course of the general argument now requires the solution, by means of the hydrodynamical theory, of a problem which may be thus enunciated (see Jamin, p. 205):—A conductor consisting of straight portions of fine wire which, being connected, form two rectangles, the planes of which make any angle with each other, is supported so as to be movable about a vertical axis with which a side of each rectangle is coincident; the horizontal sides are equal, the vertical sides are of given lengths l and l' , and between them is fixed a straight vertical rheophore of indefinite length, the current of which is opposite in direction to the currents of the vertical sides; the effect of the earth's magnetism being eliminated, and the distances a and a' of the indefinite current from the vertical sides in the case of equilibrium being measured, it is found that $\frac{l}{l'} = \frac{a}{a'}$. The solution of

this problem may be given as follows on the hydrodynamical theory.

69. From what is said in art. 67, it is not allowable in that theory to make use of any formula for the value of F differing from the formula (c) obtained in art. 63. For shortness, i and i' will be respectively put for μV and $\mu' V'$. Then, since for the action of the fixed rheophore on the movable vertical sides of the other we have for any two elements $\theta' = \pi - \theta$ and $\epsilon = 0$, the formula becomes

$$F = \frac{F_0 i i'}{r^2} \sin^2 \theta \, ds \, ds'.$$

To obtain the resultant of the action of the indefinite current on one of the movable sides of the other, it is only required to calculate $\Sigma . F \sin \theta$, the sum of the *transverse* actions, because, evidently, the resultant in the direction *parallel* to the indefinitely long current will be zero. The action in this instance is repulsive, because, by hypothesis, the currents are in opposite directions. The summation $\Sigma . F \sin \theta$ is required to embrace the transverse repulsive action of every element of the fixed rheophore on every element of the movable one. Consequently

$$\Sigma . F \sin \theta = \iint \frac{F_0 i i'}{r^2} \sin^3 \theta \, ds \, ds',$$

the integrals being taken between assignable limits. Let the rheophore acted upon be that whose length is l and distance from the fixed rheophore a , and let ds' be one of its elements. Then, supposing s to be reckoned from the foot of a perpendicular to the fixed rheophore drawn from this element, we shall have $s = a \cot \theta$; and since the distance (r) between the two elements is $a \operatorname{cosec} \theta$, we get by substitution in the formula,

$$\Sigma . F \sin \theta = \iint - \frac{F_0 i i'}{a} \sin^3 \theta \, d\theta \, ds',$$

the first integral being taken from $\theta = 0$ to $\theta = \pi$, and the other from $s' = 0$ to $s' = l$. The result will therefore be

$$\Sigma . F \sin \theta = - \frac{4F_0 i i' l}{3a}.$$

By a like process we should obtain for the repulsion on the other vertical portion of the movable rheophore

$$\Sigma . F \sin \theta = - \frac{4F_0 i i' l'}{3a'}.$$

And since in case of equilibrium these forces are equal, it follows that

$$\frac{l}{a} = \frac{l'}{a'}, \text{ or } \frac{l}{l'} = \frac{a}{a'}.$$

It is here to be remarked that in Ampère's theory the proof of the law of the inverse square is *founded upon the experimental fact*; whereas in the course of reasoning which I have adopted that law is deduced from independent *à priori* principles (see art. 57); and the above explanation of the fact may therefore be regarded as evidence of the truth of the hydrodynamical theory of the galvanic current. It may also be remarked that the above result is the same as that obtained by Ampère, although I have taken no account of the term in his formula which involves the factor k . The fact is, as the analysis in Jamin pp. 205–207 shows, the result is independent of the particular value of k , and would be the same if this factor were supposed to be zero.

70. I propose now to calculate the action of a finite rectilinear rheophore AB on another CD, also finite and rectilinear, supposing their axes to be in the same plane and inclined to each other at any angle, the latter to be fixed and the former to be susceptible of motion in that plane about a centre O situated at the intersection of the prolongation of the axes (see the figure 555 in Jamin, p. 200). The extremities C and B being those nearest to O, suppose the direction of the current in CD to be from C towards D, and that of the current in AB from A towards B; and let $OC=a$, $OD=b$, $OB=a'$, $OA=b'$, and the angle $AOD=\omega$. Also, P being the position of any element of the fixed rheophore, and P' that of an element of the movable one, let $OP=s$, $OP'=s'$, the angle $OPP'=\theta$, and the angle $OP'P=\theta'$. Then, F being the action of the element at P on that at P', the formula (c) becomes for this case

$$F = \frac{F_0 i i'}{r^2} \sin \theta \sin \theta' ds ds'.$$

As the movable rheophore is restricted to motion of rotation about O, in calculating the force tending to put it in motion we have only to take account of the resolved part perpendicular to its axis, which is $F \sin \theta'$. Hence, in order to calculate the sum of the *moments* of the forces about O, it will be required to find the value of $\Sigma . F s' \sin \theta'$, or of the double integral

$$\iint \frac{F_0 i i'}{r^2} s' \sin \theta \sin^2 \theta' ds ds',$$

the first integral being taken with respect to s , from $s=a$ to $s=b$, and the other being taken with respect to s' , from $s'=a'$ to $s'=b'$. For effecting these integrations we have the three equations

$$\frac{s}{s'} = \frac{\sin \theta'}{\sin \theta}, \quad \frac{r}{s'} = \frac{\sin \omega}{\sin \theta}, \quad r^2 = s^2 + s'^2 - 2ss' \cos \omega.$$

It is not necessary for my present purpose to go through the details of integrating, which would be a process of considerable complexity; the following deduction from the differential quantity itself will suffice for indicating, for any given inclination of the axes of the rheophores, the character of the action of the fixed rheophore which tends to cause rotation of the movable one.

71. It may, first, be observed that that differential quantity has the same sign for all values of the angle of inclination of the rheophores from $\omega=0$ to $\omega=\pi$, because θ has the same sign in all these positions. According to the assumed directions of the currents relative to the point O, V and V' and, by consequence, i and i' have different signs; and the sign of the differential is therefore *negative*. This sign is to be taken as indicating *repulsion*, because, in fact, for all the values of ω the parts of two elementary currents resolved transversely to the line joining them will be in *opposite* directions. This would also be the case if the directions of V and V' be both changed, so as to be still opposite as regards the point O. But if the currents be *both from*, or *both towards*, that point, so that V and V' have the same sign, the sign of the differential quantity would be *positive*, and the action between any two elements would be *attractive*.

72. Taking a case in which ω is very small, if the distances s and s' of two elements from O be nearly equal, r^2 would be very small, being nearly equal to $(s-s')^2$, and at the same time θ and θ' would not differ much from right angles. Hence, by the formula, the force between two elements so situated, whether attractive or repulsive, would be very great, and, if the value of r could actually be zero, would become infinite. Again, supposing ω to be nearly equal to π , r would be nearly equal to $s+s'$, θ and θ' would each be very small, and consequently the action, whether attractive or repulsive, would be very small between *any* two elements. This action would change sign with the change of sign of θ , and, therefore, if repulsive, it would be *directive*. From these considerations it may be concluded that in the case of repulsive action (for which V and V' have different signs), the fixed rheophore tends to make the other revolve from the position of maximum repulsion and unstable equilibrium, for which $\omega=0$, to the opposite position of minimum repulsion and stable equilibrium; and in the case of attractive action (for which V and V' have the same signs), the fixed rheophore tends to produce a revolution of the other from the position of minimum attraction and unstable equilibrium, for which $\omega=\pi$, to the opposite position of maximum attraction and stable equilibrium. These results agree with well-known facts.

73. I come now to the consideration of a circumstance which bears in an especial manner on the relation of the hydrodynamical theory to the theory of Ampère, and on the proof of the *à priori* character of the former. It is made evident by the investigations of Ampère and of physicists who have adopted his views, that the formula for the mutual action between two galvanic elements, which he constructed with so much labour and ingenuity, does not apply, without some additional hypothesis, to the action of a galvanic element on a *magnetic* element. (I understand a magnetic element to be the magnetism of an indefinitely small magnetized needle.) This is true also of the formula, consisting of a single term, which I have employed for the solution of the problems discussed in arts. 69–72. The hydrodynamical theory assigns as the reason for this inapplicability of the formula, that a galvanic element differs essentially from a magnetic element in respect of the mode in which *the circulation of the currents* takes place in the two cases, this condition being fulfilled in galvanism by means of conductors forming a complete circuit, whilst in magnetism, as is explained in art. 12, the circulation results from the reaction of the inertia of the fluid against the impulses of the magnet. The theory has, in fact, shown that two elements of galvanic currents are capable by their composition of producing forces which vary inversely as the *square* of the distance between the rheophore-elements to which they belong, and are proper for causing *motions of translation* of these elements, but that a stream pertaining to a galvanic element (as also one pertaining to a magnetic element) can give to an elementary *magnet*, by composition with its streams, only a *motion of rotation*, the force producing this effect varying approximately as the inverse *cube* of the distance between the elements.

74. This theory therefore accounts for the introduction into Ampère's theory of an *additional* hypothesis, having the effect of changing the law of action of a galvanic element from the inverse square to the inverse cube, when, in place of acting on an element of a rheophore, it acts upon a small magnetic needle. The case is analogous to that of the action of a large magnet on a small one (considered in arts. 21–23), in which the law of the inverse cube results from the empirical hypothesis of a mutual action, attractive and repulsive, according to the law of the inverse square, between the two magnetisms named by the Astronomer Royal *red* and *blue*. There is, however, this distinction between the two cases—that whereas the hydrodynamical theory does not indicate the existence of *two* magnetic forces varying inversely as the square of the distance, but simply the action by *pressure*, according to the law of the inverse cube, of a single fluid (the æther), the same theory accounts for the mutual action,

by the intervention of the æther, between two galvanic elements, according to the law of the inverse square (see art. 69).

75. The processes by which different physicists have calculated the action of a galvanic coil on a small magnet rest entirely on the results of experiments made by Ampère, the details of which are given in his work entitled *Théorie des Phénomènes Electrodynamiques uniquement déduite de l'expérience*. Ampère, with the view of explaining magnetic phenomena by galvanic action, conceived that each molecule of a magnet might be surrounded by a complete galvanic circuit, and by following out the consequences of this hypothesis was led to give to a galvanic conductor the form of a coil, or solenoid, the action of which is found practically to be nearly the same as that of a magnet. Advantage being taken of this experimental result, the calculation of the action of a coil on an external small magnet is reduced to the calculation of *magnetic* action. On these principles Professor Clerk Maxwell, in his 'Treatise on Electricity and Magnetism' (vol. ii. p. 131, art. 482), compares "the action of a finite [galvanic] current with that of a magnetic shell of which the circuit is the bounding edge;" and then adds, "If the circuit be supposed to be filled up by a surface bounded by the circuit and thus forming a diaphragm, and if a magnetic shell coinciding with this surface be substituted for the electric [*i. e.* galvanic] current, then the magnetic action of the shell on all distant points will be identical with that of the current." This conclusion entirely depends on the assumption that the magnetic shell has a *negative* side as well as a *positive* side, which is equivalent to assuming the simultaneous action of the *red* and *blue* magnetisms mentioned above. In consequence of this hypothesis the law of the magnetic action of any element of the shell is changed from the inverse square to the inverse cube, and is thus made to accord with the law deduced from the *à priori* principles of the hydrodynamical theory.

76. In the 'Treatise on Natural Philosophy,' by Professor Sir William Thomson and Professor Tait, the following passage occurs in vol. i. § 477 (*e*):—

"For magnetic and electromagnetic applications a very useful case [of the action according to the law of the inverse square] is that of two equal disks, each perpendicular to the line joining their centres, on any point in that line—their masses being of opposite sign—that is, one repelling and the other attracting. Let a be the radius, ρ the mass of a superficial unit of either, c their distance, x the distance of the attracted point from the nearest disk. The whole action is evidently

$$2\pi\rho\left\{\frac{x+c}{((x+c)^2+a^2)^{\frac{3}{2}}} - \frac{x}{(x^2+a^2)^{\frac{3}{2}}}\right\}.$$

In the particular case when c is diminished without limit, this becomes

$$2\pi\rho c \frac{a^2}{(x^2 + a^2)^{\frac{3}{2}}},$$

It will hence be seen that by this reasoning the inverse cube of the distance is arrived at in the same manner as by that indicated in arts. 21 and 22 relative to Gauss's empirical theory of magnetism.

In the same work (§ 546 (III.)) the formula for "the potential U of a disk of infinitely small thickness c with positive and negative matter of surface-density $\frac{\rho}{c}$ on its two sides," as obtained by inference from the above expression, is

$$2\pi\rho \left(1 - \frac{x}{(x^2 + a^2)^{\frac{1}{2}}}\right),$$

the point Q acted upon being situated on the perpendicular to the disk through its centre C . For cases in which Q is at any distance r from C greater than a , and QC makes an angle θ with the perpendicular, the formula for U is found to be

$$U = 2\pi\rho \left(\frac{1}{2} \frac{a^2}{r^2} Q_1 - \frac{1 \cdot 3}{2 \cdot 4} \frac{a^4}{r^4} Q_3 + \&c. \right),$$

the factors $Q_1, Q_3, \&c.$ being functions of θ which satisfy the equality

$$(1 - 2e \cos \theta + e^2)^{-\frac{1}{2}} = 1 + Q_1 e + Q_2 e^2 + Q_3 e^3 + \&c.$$

The formula thus obtained for the potential U is the one used by Mr. Stuart in the calculations already referred to in arts. 51 and 54. If r be considerably greater than a , the first and principal term of $\frac{dU}{dr}$ varies inversely as r^3 ; and, as before, this result depends on the hypothesis of positive and negative magnetic matter on the two sides of the disk.

77. Having given the foregoing explanations, I have now only a few additional remarks to make. Whilst the hypothesis by means of which the law of the inverse cube has been *experimentally* educed, as respects both magnetic and galvanic action, is entirely *empirical*, arising out of no antecedent reasoning, in the hydrodynamical theory, on the contrary, that law is arrived at by mathematical deduction from initial principles; and this is true both in the theory of magnetic action and in that of galvanic action, inasmuch as the expressions for the two parts, direct and indirect, which the latter action was found to consist of, both vary, in the first and principal term, according to the law of the inverse cube. Thus, since the hypothesis that a complete circular

rheophore acts as a sheet or shell of positive and negative magnetic matter enclosed by the circuit leads to the same law, it follows that the *à priori* theory gives a *reason* for that hypothesis. Besides, from a consideration of the modes of the direct and indirect actions of the coil discussed in arts. 45–52, it would seem that the hypothesis is applicable with more exactness to the action of a coil than to that of a magnet.

78. Since, therefore, the hydrodynamical theory has given a reason for the empirical hypothesis on which the formula and calculations referred to in art. 76 are founded, and has also accounted for all the experimental facts from which Ampère's theory is deduced, I think I am entitled to say that, so far as the comparison of calculated results with experiment is satisfactory, it is to be regarded as confirmatory of the *hydrodynamical* theory. At the same time, as I have indicated in art. 53, and proved in the Postscript to Part II., that theory admits of being compared with experiment quite apart from any reference to Ampère's theory.

In concluding this communication, I feel justified in expressing the opinion that the arguments in Parts I., II., and III. suffice for concluding that the theoretical explanation of galvanic and magnetic phenomena is to be sought for by means of mathematical deductions from the fundamental principles enunciated in art. 1—and that, in so far as the present essay has been successful in this respect, it carries with it the whole of the theoretical philosophy I have laboured so many years to uphold and advance, which I consider to be, in fact, the only legitimate extension of the Newtonian philosophy. It has sometimes been said that Ampère is the Newton of galvanic and magnetic physics. But since Ampère himself professes that he makes deductions exclusively from experiment, he ought rather to be compared with Kepler than with Newton. After all that he did, there remained to be attempted in general physics the performance of the part analogous to that in physical astronomy which was begun by Newton and has since been so successfully carried on. It is moreover to be said that the foundation of the science of theoretical physics is laid in Newton's *Principia*, this work being by no means confined to physical astronomy, but comprehending the principles of all departments of natural philosophy which have relation to the laws of action of *physical force*. It has been in conformity with rules and principles of philosophy that Newton laid down, and for the purpose of applying them in researches respecting the physical forces and the constitution of bodies which the state of science in his day did not allow of undertaking, that my efforts have been made in the department of theoretical physics; and I now take occasion to state that the main con-

clusions relative to the *modus operandi* of the physical forces to which this system of philosophy seems to point are :—that they are all modes of *pressure* of the æther ; that the forces concerned in light, heat, molecular attraction and repulsion, and gravity are dynamical results of *vibrations* of the æther ; and that electric, galvanic, and magnetic forces are due to its pressure in *steady motions*.

Cambridge, October 16, 1874.

LVIII. *Researches in Acoustics.*—No. V.

By ALFRED M. MAYER.

[Continued from p. 385.]

4. *Suggestions as to the Function of the Spiral Scalæ of the Cochlea, leading to an Hypothesis of the Mechanism of Audition.*

AS the auditory nerve has by far its highest development in the cochlea, it is a natural inference that this part of the ear is chiefly concerned in audition, and that the very peculiar form of the cochlea fulfils some important function ; yet the relations of this form to the mode of audition have occupied but little the attention of physiologists. The only suggestion as to the uses of its form with which I am acquainted is that given by Dr. J. W. Draper in his 'Physiology,' N. Y., 1855. This distinguished philosopher states that "it may be imagined how it is that a sound passing through the auditory canal, the bones of the tympanum, the membrane of the fenestra ovalis, and thus affecting its destined portion of the lamina, does not give rise to an idea in the mind of repetition or reverberation by moving back and forth through the two scalæ and affecting its proper nerve-fibril at each passage. Is there not a necessity for the exertion of some mechanism of interference which shall destroy the wave after it has once done its work ?" Dr. Draper then reasons that this reverberation is prevented by the scalæ being of different lengths and by the fact of their junction in the helicotrema. These two circumstances give rise to interferences in the helicotrema of the waves which have proceeded from the stapes up the scala vestibuli with the waves which passed from the membrana tympani across the tympanum to the fenestra rotunda, and thence up the scala tympani to the helicotrema. Dr. Draper also states that when the stapes is pushed in by the contractions of the tensor tympani and stapedius muscle, the relative length of the scalæ is changed, and thus the proper adjustment for an interference is effected. But even granting that

"reverberations or repercussions" take place in a body like the whole apparatus of audition, whose heterogeneous structure must make all of its vibrations, *taken as a mass*, forced oscillations, I do not agree with my distinguished friend in thinking that the difference in the lengths of the *scalæ* could bring about any interference except of the most minute and inefficient amount; even if we could agree with Dr. Draper that the intensity of the pulses sent from the *fenestra rotunda* nearly equal the intensity of those sent up the *scala vestibuli* from the stapes. The following considerations will make clear our objections to the hypothesis of Dr. Draper. If we take the mean wave-length of the sounds which fall upon the ear as that of the treble G of 440 vibrations per second, it follows that this wave-length will be 1 metre. But the velocity of sound in the fluid of the *scalæ* is at least $4\frac{1}{4}$ times what it is in air of the same temperature; therefore the average length of the sonorous waves which traverse the *scalæ* is $4\frac{1}{4}$ metres; and hence, for two such waves meeting in the *helicotrema* to completely interfere, one *scala* would have to exceed the other in length by 2.12 metres. But the entire length of a *scala* is at the highest only 29 millims.; and the difference in their length, taken at its maximum, is so slight that the diminution in the intensity of the resultant wave produced in the *helicotrema* is inappreciable; and especially will it be so considered when we take into account the relatively feeble intensity of the wave which is sent from the tympanic membrane across the air of the drum on to the membrane of the *fenestra rotunda*, where two sudden changes in density occur before it passes up the *scala tympani*.

The following attempt at an explanation of the functions of the spiral stairways of the cochlea is given merely as a suggestion, and with the hope that I may thereby call the attention of students of physiological acoustics to the consideration of the uses of these peculiar forms. Recent studies in embryology and comparative anatomy have shown that the *ductus cochlearis* is the essential part of the ear, and that the forms of the *scalæ* are determined by it; for "the original soft parts of the cochlea are distinct from their osseous capsule, which belongs to the petrous bone; the *scalæ* are secondary formations around the principal canal of the cochlea, the *ductus cochlearis*, whose epithelial lining proves eventually to be the germ-centre, so to speak, of the entire apparatus"*.

The fact that the *ductus* controls the form of the *scalæ*, and not *vice versa*, shows that the *scalæ* must bear some very important functional relation to the *ductus*. This relation will become evident on considering the

* Waldeyer, "On the Auditory Nerve and Cochlea," in Stricker's 'Histology.'

actions which must take place when a sound-wave traverses the scalæ.

All know that the organ of Corti is enclosed in the ductus cochlearis—a canal of triangular section bounded on two of its sides by the scalæ, and on its third by the membranes lining the outer wall of the cochlea. The upper wall of this canal is formed by the membrana Reissneri, which separates it from the scala vestibuli; and its lower wall is the lamina spiralis and the elastic membrana basilaris which separate it from the scala tympani. The ductus is closed at its upper end, and at its lower end it communicates with the sacculus hemisphæricus by a fine duct. The arch of Corti rests upon the membrana basilaris, which extends beyond the base of the arch to the membranous outer wall of the cochlea; and over the arch spreads the membrana tectoria, covering the rods of Corti and the hair-cell cords as with a roof, but leaving the outer portion of the elastic membrana basilaris exposed. We will now show that the significance of these anatomical relations is to bring the sound-vibrations to act with the greatest advantage on the co-vibrating parts of the ear, and to cause these parts to make one half as many vibrations in a given time as the tympanic or basilar membranes.

The relations which the form of the scalæ bears to the sonorous waves traversing them, will be modified according to the existence or non-existence of a communication between the scalæ. On this point there seems to be some difference of opinion*; and therefore I will attempt to explain the functions of the scalæ first on the supposition that they are continuous, and then on the assumption that they are not continuous, but closed at the place where the helicotrema is supposed by most anatomists to exist.

E. Weber was the first to point out the peculiar molecular actions which exist when the dimensions of a body are very small compared with the length of the sonorous waves which traverse it; and Helmholtz based his investigations on the Mechanism of the Ossicles of the Ear on the theory of Weber, which Helmholtz gives in these words:—"The difference in displacement of two oscillating particles whose distance from one another is infinitely small compared with the wave-length, is itself

* Dr. Albert H. Buck, of New York, writes me:—"that in 1870 I undertook to demonstrate the existence of the helicotrema by making sections through the cochleæ of an infant at right angles to the base. In this I utterly failed; and although able to obtain a good view of the last part of the dividing septum, both from below and from above, neither Professor Helmholtz nor I could detect the slightest trace of an opening. Of course it would not do to base my denial of the existence of the helicotrema upon the results of but a single investigation."

infinitely small compared with the entire amplitude of displacement." It is evident that the compressions and dilatations which may exist in any body depend entirely on the differences in the phases of the vibrations constituting the sonorous wave; and when the body has a depth equal to half a wave-length, it can embrace the maximum amount of condensation and rarefaction. But condensation and dilatation alone produce *lateral action* on the walls of a straight canal traversed by sonorous vibrations; and hence, if the length of the canal be but a small fraction of the wave, then there exists throughout the canal but little difference in phase, and therefore but little lateral action. Now the united lengths of the scalæ is but a small fraction of the mean length of the sonorous waves which traverse it; for if we take, as above, $4\frac{1}{2}$ metres as the mean length of the waves which are propagated through the scalæ, and 59 millims. as the length of the united scalæ, it follows that the latter is only $\frac{1}{72}$ of the mean wave-length. Now if we imagine the scalæ straightened and forming one continuous tube with a free communication existing at the helicotrema, then the mean wave traversing them will cause only $\frac{1}{22}$ of the lateral action which this same wave would produce if the scalæ had the length of half of the wave; and it follows that the whole liquid of the scalæ would vibrate forward and backward almost as an incompressible mass, approaching in character to the oscillations of a solid piston in a cylinder; therefore the action against the walls of the ductus cochlearis would be very slight. But now consider the change in effect on the ductus which takes place when it, together with the scalæ, is wound up into such an ascending spiral as exists in the ear. The molecules of the liquid in the scalæ thrown forward and backward by the vibrations of the stapes, tend to move in straight lines; but the now curved form of the scalæ causes them to press against the outer or peripheral part of the upper wall (*membrana Reissneri*) of the ductus cochlearis and against the outer part of the lower wall (*membrana basilaris*) when the stapes moves inward; and when it moves outward this action of compression is relieved from the two opposite walls of the ductus. But these actions produced by the stapes on the two walls of the ductus are opposed to each other; and since they take place simultaneously and with about the same intensity (by reason of our assumption of the free communication of the scalæ), the rods of Corti and the hair-cells will not vibrate, but will only experience compressions and dilatations like the fluid in which they are immersed. Therefore there appears to me a physical basis for the opinion that either there is no communication between the scalæ, or, if the helicotrema exists, that it must be a very constricted passage. Indeed, if we

adopt the latter view, then every thing works to produce the maximum effect on the covibrating parts of the organ of Corti; for when the stapes moves inward, the pressure is thrown on the outer border of the upper wall or roof of the ductus, thence across to the peripheral portion of the basilar membrane. This action, we may say, takes place simultaneously throughout the whole length of the ductus, moves downward the floor of the basilar membrane, and thus presses the fluid of the scala tympani against the fenestra rotunda and moves this membrane outward. When, however, the stapes moves outward, the pressure is relieved from the elastic basilar membrane, which is now moved upward, while the fenestra rotunda moves inward*.

There are also other anatomical facts, besides the inclination of the membrana Reissneri to the plane of the membrana basilaris, and the inclination of both these membranes to the plane perpendicular to the axis of the cochlea, which favour an opinion that the outer or peripheral part of the basilar membrane receives the main part of the vibrations which enter the ductus. The auditory nerve-fibrils are not attached to the Corti rods or pillars, as was formerly imagined; and therefore these bodies cannot be the covibrating parts of the ductus; but the Corti pillars appear to act, in conjunction with the cylindrical nerve-cells of Hensen, as supports for the lamina reticularis, between which and the basilar membrane are steadily and tensely stretched the hair-cell *cords* (as I will term them); and to these cords are attached the nerve-fibrils. Waldeyer says on this point that "The outer radial fibres direct their course, as Gottstein has found, toward the tunnel of Corti, passing between the inner pillars and traversing the tunnel about midway between the summit and base of the arch; in a profile view these fibres appear like stretched harp-strings. On leaving the arched space they pass between the outer pillars and direct their course (rising a little toward the scala vestibuli) straight to the hair-cells, with which they become completely fused. In several preparations from the dog and the bat I have seen this termination of the nerves in the most convincing manner, at least so far as the innermost row of hair-cells is concerned; as to the other rows, we may pretty confidently assert that the termination of the nerves is the same; for we can frequently see several fibres passing at the same time between the outer pillars." The very fact that the number of these hair-cell cords increases with the higher development of the ear shows their important

* If we could examine at the same time vibrating points on the stapes and on the fenestra rotunda with a vibration-microscope, I imagine that these points would exhibit no difference in phase when the membrana tympani vibrated to a note below the treble.

function; for while in man they are arranged alternately in five rows and number 18,000, in other Mammalia there are only two or three rows*. These hair-cell cords are more perpendicular to the basilar membrane than the Corti rods, and are also different in their forms, having swellings in the middle of their lengths. These swellings must cause them to act like loaded strings; and hence each hair-cell cord is peculiarly well adapted to covibrate with only one special sound. Also these hair-cell cords are placed in reference to the sound-pulses striking them somewhat in the relation which the antennal fibrils of the mosquito bear to a wave-surface to which their lengths are perpendicular. The hair-cell cords, therefore, will not be set in vibration by the action of the feeble pulses which may reach them directly through the membrana Reissneri from the scala vestibuli; and, furthermore, the shielding influence of the membrana tectoria tends to prevent this direct action on the cords.

If my view be correct, that these cords receive their vibrations from the basilar membrane, and not directly from the impulses sent into the ductus, it necessarily follows that these cords bear, to the membrane to which they are attached, the same relation as stretched strings bear to the vibrating tuning-forks in Melde's experiments; and therefore *a cord in the ductus will vibrate only half as often in a second as the basilar membrane to which it is fastened*. Experiments similar to those described in section I of this paper illustrate very well our hypothesis of audition. Thus the membrane placed near the sounding-reed stands for the basilar membrane; strings of various lengths and diameters and loaded at their centres are fastened to the membrane, and represent the hair-cell cords. On sounding the reed-pipe, only those strings in tune with the harmonics existing in the composite sound of the reed will enter into vibration, just as, when the same sound-vibrations enter the ear and vibrate the basilar membrane, the only hair-cell cords which enter into vibration are those in tune with the elementary vibrations existing in the membrane. Also it is to be observed that as the loaded string makes one vibration to two of the membrane, so the hair-cell cord makes only one vibration to two of the basilar membrane.

* It is to be regretted that no accurate measures of the length and diameters of the rods and cords of the organ of Corti have been secured. The outer pillars of the arch of Corti certainly double their length in going from the base to the top of the ductus; but does this fact point them out as bodies suitably proportioned to covibrate to sounds extending through at least *eight octaves*? I know of no measures on the hair-cell cords. When their dimensions are determined, physiologists will be able to give more precision to their hypotheses.

If it be true that when simple vibrations impinge on the ear the tympanic and basilar membranes vibrate twice while the co-vibrating body only vibrates once, then it follows that, if the same simple vibrations can be sent directly to the co-vibrating parts of the ear without the intervention of the basilar membrane, we should perceive a sound which is the octave of the one we experienced when the same simple vibrations entered the ear through the tympanic membrane. Hence it appears that our hypothesis can be brought to the test of experiment in the following manner:—A tuning-fork held near the ear causes a sensation corresponding to the designated pitch of the fork. But the vibrations of this fork can be sent to the inner ear through the bones of the head; and although we cannot prevent the simultaneous vibration of the tympanic and basilar membranes, yet we can at the same time directly vibrate the parts of the inner ear. Therefore, if we first hold this fork near the ear and note its pitch and the quality of its sound and then press its foot firmly against the temporal bone, we should perceive a marked difference in the timbre of the fork when sounded in these two different positions; for when its foot is against the head, we should hear the usual simple sound of the fork accompanied by its octave.

Thus, if we take an Ut_3 fork and vibrate it near the ear and closely apprehend the character of its sound, we shall experience a sensation which certainly does not contain that corresponding to the higher octave of the fork. Now press firmly the foot of the fork against the zygomatic process close to the ear, directing the foot of the fork somewhat backward, and we shall distinctly hear the higher octave of the fork singing in concert with its real note. If the auditory canal be now closed by gently placing the tip of the finger over it, we shall perceive the higher octave with an intensity almost equal to that of the fundamental note. The same sensation, though less intense, may be obtained by placing the fork on any part of the temporal bone. One can also perceive distinctly the higher octave when the fork is placed on the parietal bone, about two inches in front and an inch or so to the side of the foramen, with its foot directed toward the opposite inner ear, while the auditory canal of this ear is gently closed with the tip of the finger. But the higher octave sings out with the greatest intensity when the foot of the fork is placed on the tragus of the outer ear. A friend, who is a musician as well as a physicist, repeated these experiments; and he informs me that when the foot of the fork is placed against the tragus of his ear he hears the higher octave to the almost entire exclusion of the lower, and with a clearness that reminds him of the sensation perceived when an Ut_4 resonator, placed to the ear, rein-

forces its proper note. The higher octaves of several forks have been thus perceived; but the forks from Ut_3 to Ut_4 , inclusive, appear to give the best results.

The fact that sound-pulses sent to the inner ear through the head give the sensation corresponding to the higher octave of that perceived when the fork vibrates the air outside the ear, and therefore that different covibrating parts of the ear are set in action by the vibrations reaching the ear by these two different routes, is a necessary consequence of my hypothesis of the mode of audition, and was not suspected until my hypothesis pointed it out to me, and was not known until I attempted to test the hypothesis by experiment. I know of no other hypothesis that accounts for this fact, which, while it is a necessary consequence of my own views, is directly opposed to those hypotheses hitherto formed on the mode of audition; for, according to the latter, the covibrating parts of the ear make as many oscillations in a given interval as the tympanic and basilar membranes.

[To be continued.]

LIX. *A Statical Theorem.* By LORD RAYLEIGH, M.A., F.R.S.*

IN a paper "On some General Theorems relating to Vibrations," published in the Mathematical Society's 'Proceedings' for 1873, I proved a very general reciprocal property of systems capable of vibrating, with or without dissipation, about a position of stable equilibrium. The principle may be shortly, though rather imperfectly, stated thus:—If a periodic force of harmonic type and of given amplitude and period act upon the system at the point P, the resulting displacement at a second point Q will be the same both in amplitude and phase as it would be at the point P were the force to act at Q.

If we suppose the period of the force to be very great, the effects both of dissipation and inertia will ultimately disappear, and the system will be in a condition of what is called movable equilibrium; that is to say, it will be found at any moment in that configuration in which it would be maintained at rest by the then acting forces, supposed to remain unaltered. The statical theorem to which the general principle then reduces is so extremely simple that it can hardly be supposed to be altogether new; nevertheless it is not to be found in any of the works on mechanics to which I have access, and was not known to the physicists to whom I have mentioned it. In any case, I think, two or three pages may not improperly be devoted to the consideration of it.

* Communicated by the Author.

Let the system be referred to the independent coordinates ψ_1, ψ_2 , &c., reckoned in each case from the configuration of equilibrium. Since only small displacements are contemplated, ψ_1 &c. are small quantities whose squares are to be neglected. Then, if Ψ_1, Ψ_2 , &c. are the impressed forces of the corresponding types, the equations of equilibrium are of the form

$$\left. \begin{aligned} a_{11}\psi_1 + a_{12}\psi_2 + a_{13}\psi_3 + \dots &= \Psi_1, \\ a_{21}\psi_1 + a_{22}\psi_2 + a_{23}\psi_3 + \dots &= \Psi_2, \\ a_{31}\psi_1 + a_{32}\psi_2 + a_{33}\psi_3 + \dots &= \Psi_3, \\ \dots &\dots \end{aligned} \right\} \dots \dots \quad (A)$$

being the same in number as the degrees of freedom. It may be observed that forces of a constant character need not be included in Ψ_1 &c.; for the effect of such is only to alter the configuration of equilibrium, and may be supposed to be already accounted for in the estimation of that configuration.

If the system be conservative, as is here supposed, the coefficients in the equations (A) are not all independent; for in order that an energy-function may exist, any coefficient such as a_{rs} must be equal to the corresponding a_{sr} .

The solution of equations (A) is

$$\left. \begin{aligned} \nabla\psi_1 &= A_{11}\Psi_1 + A_{12}\Psi_2 + \dots, \\ \nabla\psi_2 &= A_{21}\Psi_1 + A_{22}\Psi_2 + \dots, \\ \nabla\psi_3 &= A_{31}\Psi_1 + A_{32}\Psi_2 + \dots, \\ \dots &\dots \end{aligned} \right\} \dots \dots \quad (B)$$

where ∇ denotes the determinant

$$\begin{vmatrix} a_{11} & a_{12} & a_{13} \dots \\ a_{21} & a_{22} & a_{23} \dots \\ a_{31} & a_{32} & a_{33} \dots \\ \dots & \dots & \dots \end{vmatrix}$$

and

$$A_{11} = \frac{d\nabla}{da_{11}}, \quad A_{12} = \frac{d\nabla}{da_{12}}, \quad \&c.,$$

in which, therefore, by a property of determinants,

$$A_{rs} = A_{sr}.$$

In the application that we are about to make it will be supposed that all the forces but two vanish, for example that Ψ_3, Ψ_4 , &c. vanish. Under these circumstances we obtain from (B)

$$\left. \begin{aligned} \nabla\psi_1 &= A_{11}\Psi_1 + A_{12}\Psi_2, \\ \nabla\psi_2 &= A_{21}\Psi_1 + A_{22}\Psi_2, \end{aligned} \right\} \dots \dots \quad (C)$$

equations which determine the displacements ψ_1, ψ_2 when the forces are given. The consequences which follow from the fact that $A_{12} = A_{21}$ may be exhibited in three ways.

First Proposition.—Suppose $\Psi_2 = 0$. From the second equation, we see that $\nabla\psi_2 = A_{21}\Psi_1$. Similarly if we had supposed $\Psi_1 = 0$, we should get $\nabla\psi_1 = A_{12}\Psi_2$, showing that the relation of ψ_1 to Ψ_2 in the second case is the same as the relation of ψ_2 to Ψ_1 in the first.

In order to fix our ideas we will take the case of a rod, not necessarily uniform, supported in any manner in a horizontal position—for example, with one end clamped and the other free. Then, if P and Q be any two points of its length, we assert that a pound weight hung on at P will give the same linear deflection at Q as is observed at P when the weight is hung at Q; and the only thing on which our conclusion depends is the proportionality of strains and stresses. If we take angular instead of linear displacements, the theorem will run:—A given couple at P will produce the same rotation at Q as the couple at Q would give at P. Or if one displacement be linear and the other angular, the result may be stated thus:—A couple at P would do as much work in acting over the rotation at P due to a simple force at Q, as the force at Q would do in acting over the linear displacement at Q due to the couple at P. In the last case the statement is more complicated, since the forces, being of different kinds, cannot be made equal.

Second Proposition.—Suppose that $\psi_1 = 0$. Then, from (C),

$$A_{12}\nabla\psi_2 = (A_{12}^2 - A_{11}A_{22})\Psi_1.$$

From this we conclude that if ψ_2 is given, it requires the same force Ψ_1 to keep $\psi_1 = 0$, as would be required in Ψ_2 to keep $\psi_2 = 0$, if ψ_1 had the given value.

Thus, if the rod be supported at P so that that point cannot fall, while Q is depressed one inch by a force there acting, the reaction on the support at P is the same as it would have been on a support at Q if P had been depressed one inch*.

Third Proposition.—Suppose, first, that $\Psi_1 = 0$. Then, from (C),

$$\psi_1 : \psi_2 = A_{12} : A_{22}.$$

Secondly, suppose $\psi_2 = 0$. Then

$$\Psi_2 : \Psi_1 = -A_{12} : A_{22}.$$

Thus, when Ψ_2 alone acts, the ratio of displacements $\psi_1 : \psi_2$ is the negative of the ratio of the forces $\Psi_2 : \Psi_1$ necessary to keep $\psi_2 = 0$.

If the rod is supported at P and bent by a force acting down-

* The verification of these results with rods variously supported, or more complicated structures, gives a very good experimental exercise.

wards at Q, the reaction bears the same ratio to the force as the displacement at Q would bear to the displacement at P when the unsupported rod is bent by a force applied at Q.

In this proposition the interchange of P and Q gives a different, though of course an equally true statement. The first two propositions are themselves reciprocal in form.

The second and third propositions, as well as the first, admit of the extension to the vibrations of systems subject to inertia and dissipation ; but I do not here pursue this part of the subject.

Our fundamental equations (C) may be arrived at with less analysis and perhaps equal rigour by a somewhat modified process. The conditions that the forces $\Psi_3, \Psi_4, \&c.$ vanish, impose linear relations on the coordinates, and virtually reduce the degrees of freedom enjoyed by the system to two. But for only two independent coordinates we have at once

$$\left. \begin{aligned} \Psi_1 &= b_{11}\psi_1 + b_{12}\psi_2, \\ \Psi_2 &= b_{21}\psi_1 + b_{22}\psi_2, \end{aligned} \right\} \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (D)$$

where the coefficients b_{12}, b_{21} are equal. The equality of the coefficients b_{12}, b_{21} is a consequence of the existence of an energy-function, or may be proved *de novo* by taking the system round the cycle of configurations represented by the square whose angular points are

$$\left. \begin{aligned} \psi_1 &= 0 \\ \psi_2 &= 0 \end{aligned} \right\} \quad \left. \begin{aligned} \psi_1 &= 0 \\ \psi_2 &= 1 \end{aligned} \right\} \quad \left. \begin{aligned} \psi_1 &= 1 \\ \psi_2 &= 1 \end{aligned} \right\} \quad \left. \begin{aligned} \psi_1 &= 1 \\ \psi_2 &= 0 \end{aligned} \right\}.$$

From (D) we may deduce the three propositions directly, or mediately with the aid of (C), which is merely the algebraic solution of (D).

Finally, I would remark that essentially the same method, though with a somewhat different interpretation, is applicable to systems other than those contemplated in the preceding demonstrations. In thermodynamics the condition of a body is regarded as depending on two independent coordinates such as the temperature and volume ; and by the principles of that subject it is known that a function of that condition exists, representing the work that can be got out of the system in reducing it to a standard condition of volume and temperature, any communication or abstraction of heat being made at the standard temperature. The simplest course that can be taken is along an adiabatic up to the standard temperature, and then along the isothermal until the standard volume is attained. If the actual condition of the body be defined by $v + dv, t + dt$, while the standard condition corresponds to v, t , we have for the available energy, or entropy ($d\epsilon$),

$$2d\epsilon = dp \, dv + d\phi \, dt,$$

where dp is the variation of pressure, and $d\phi$ the variation of the thermodynamic function.

In this equation dv , dt correspond to ψ_1 , ψ_2 , dp , $d\phi$ to Ψ_1 , Ψ_2 , and $d\epsilon$ corresponds to the potential energy of the purely mechanical system. Our first proposition shows that, if $d\phi=0$, $\frac{dt}{dp}$ has the same value as that of $\frac{dv}{d\phi}$ when there is no variation of pressure, the interpretation of which is that the heat (measured as work) necessary to increase the volume by unity at constant pressure, is numerically equal to the product of the absolute temperature into the increase of pressure required to raise the temperature one degree when no heat is allowed to escape. (See Maxwell's 'Heat,' p. 167.) In like manner the other propositions may be interpreted.

LX. *Note on Carbon-Spectra. By W. MARSHALL WATTS, D.Sc., Physical-Science Master in the Giggleswick School*.*

IN the Philosophical Magazine for October 1869 I described four different spectra as spectra of *carbon*. One of them was the ordinary spectrum from hydrocarbon-flames first described by Swan; the second was the spectrum obtained from vacuum-tubes enclosing carbonic oxide, carbonic anhydride, or olefiant gas; the third was the spectrum of the Bessemer-flame, and the fourth the spectrum of the high-tension spark in carbonic anhydride or carbonic oxide

I have since shown (Phil. Mag. February 1873) that the Bessemer-spectrum, instead of being a spectrum of carbon, is the spectrum of manganese oxide; and I have now to add the result of recent observations which show that the second spectrum also is due not to carbon itself, but to some *oxide* of carbon. This spectrum was held to be a spectrum of carbon because it was common to compounds of carbon with hydrogen and with oxygen. Following the line of research indicated by Schuster (Proc. Roy. Soc. June 1872), I have now found that it is not given by spectral tubes enclosing olefiant gas if special care be taken to exclude all trace of oxygen. A description of one experiment will suffice. The olefiant gas prepared from alcohol and sulphuric acid passed first through a washing-bottle containing sulphuric acid, then through a second bottle filled with solid caustic potash. This bottle was also provided with a separating-funnel, the tube of which passed through a third hole in the cork nearly to the bottom. When the olefiant gas had passed through the whole apparatus for

* Communicated by the Author.

some time, a strong solution of pyrogallic acid was introduced through the separating-funnel into the bottle, and the tap of the funnel was closed again. The gas thus freed from oxygen, passed on through a calcium-chloride drying-tube into the vacuum-tube which formed the upper part of a tube some 300 millims. long, the lower end of which just dipped below the surface of mercury in a bottle, passing air-tight through the cork. A second tube through the cork of this bottle put it in communication with the air-pump. The vacuum-tube was thus cut off by a sort of mercury-valve from the air-pump, so that it was impossible for air to leak into the tube from the air-pump during the process of exhaustion. Finally, one end of the vacuum-tube contained sodium, and the other a minute fragment of potassium chlorate wrapped up in platinum-foil.

The gas having been allowed to pass briskly through the apparatus for some time, the vacuum-tube was melted together at the end remote from the air-pump, exhausted, and sealed off. The sodium was then heated to ensure the absence of oxygen.

The discharge from the induction-coil was then sent through the tube. The light emitted (which was not very bright), examined with the spectroscope, gave the lines of the first carbon-spectrum, the positions of the lines 5635 and 5165 being verified. The potassium chlorate was then heated so as to set free oxygen, and the observations were repeated. The light was now much brighter, and gave the *second* carbon-spectrum, the lines 5803, 5602, 5195, 4834, 4505, and 4395 being verified. We have therefore only one spectrum which can be proved to be due to *carbon*—that, namely, which is common to the flame of olefiant gas or cyanogen, the electric discharge in cyanogen or carbonic oxide at the ordinary pressure, and to the electric discharge in vacuum-tubes enclosing cyanogen, olefiant gas, or hydrocarbons such as benzol.

LXI. *On the Problem of the Eight Queens.*

By J. W. L. GLAISHER, M.A.*

THE problem referred to in the title, viz. to determine the number of ways in which eight queens can be placed on a chessboard so that no one can take (or be taken by) any other, was proposed by Nauck to Gauss, and formed the subject of a correspondence between the latter and Schumacher. Gauss, after finding the number to be 76 and then 72, ultimately arrived at 92, which has since been recognized as the correct solution. An

* Communicated by the Author.

interesting account of the history of the problem is given in the last Number of Grunert's *Archiv der Mathematik und Physik* by Dr. Siegmund Günther*, who has also suggested a new way of solving the question, which it is the object of this communication to develop.

Dr. Günther (considering a board of n^2 squares, and of course n queens) remarks that if the determinant

$$\begin{vmatrix} a_1 & c_2 & e_3 & g_4 & k_5 & . & . \\ b_2 & a_3 & c_4 & e_5 & g_6 & . & . \\ d_3 & b_4 & a_5 & c_6 & e_7 & . & . \\ f_4 & d_5 & b_6 & a_7 & c_8 & . & . \\ h_5 & f_6 & d_7 & b_8 & a_9 & . & . \\ . & . & . & . & . & . & . \\ . & . & . & . & . & b_{2n-2} & a_{2n-1} \end{vmatrix}$$

be expanded, and all the terms be rejected in which either the same letter or the same suffix appears more than once, then the terms that remain will give all the solutions of the problem. The reason for the rule is evident: from the nature of a determinant each term involves one constituent from each row and one from each column, and the terms thus represent all the positions in which the queens cannot take one another castle-fashion; the omission of the terms in which the same letter or suffix appears more than once excludes the cases in which two or more queens lie on the same diagonal (*i. e.* can take one another bishop-fashion), so that the terms that remain are the solutions. Dr. Günther develops the determinants for boards of 9, 16, and 25 squares, but, owing to the number of terms involved, does not proceed further; he remarks that for the chessboard of 64 squares it would be necessary to calculate 20,160 terms.

Of course it would be quite out of the question to actually write down twenty thousand terms; but in the way which will now be explained, I found it a lengthy piece of work certainly, but still not a very laborious task, to find by means of Dr. Günther's principle all the solutions for boards of 36, 49, and 64 squares.

Starting from the beginning, it is easily seen that on a board of 9 squares there is no solution, and that on a board of 16 squares there are two solutions, viz. $c_2e_5d_3b_6$ and $e_3b_2c_6d_5$. To find the solutions for a board of 25 squares,

* *Zur mathematischen Theorie des Schachbretts*, vol. lvi. part 3, pp. 281-292.

$$\begin{vmatrix} a_1 & c_2 & e_3 & g_4 & k_5 \\ b_2 & a_3 & c_4 & e_5 & g_6 \\ d_3 & b_4 & a_5 & c_6 & e_7 \\ f_4 & d_5 & b_6 & a_7 & c_8 \\ h_5 & f_6 & d_7 & b_8 & a_9 \end{vmatrix} \quad . \quad . \quad . \quad . \quad (1)$$

(or, say, the five-solutions) we may proceed as follows. To obtain those that involve a_9 we have only to append a_9 to every four-solution that does not involve an a : it happens that neither of the four-solutions does involve an a , so that $c_2e_5d_3b_6a_9$ and $e_3b_2c_6d_5a_9$ are the two solutions that involve a_9 . Evidently the two that involve a_1 are found by replacing each suffix by its complement to 10, and are therefore $c_8e_5d_7b_4a_1$ and $e_7b_8c_4d_5a_1$. To obtain the corresponding solutions involving h_5 and k_5 we have only to "reflect" the four solutions we have obtained (*i. e.* replace in them the r th square from the right in any row by the r th square from the left); we thus get

$$g_4a_3e-b_6h_5, \quad e_3g_6b_4a_7h_5, \quad f_4a_3d_7c_6k_5, \quad d_3f_6c_4a_7k_5.$$

These are the only solutions in which corner squares are involved (or, say, the ultimate solutions); and in developing the determinant we may replace the four corner squares by zeros. The actual development in effect is

$$\begin{vmatrix} . & c_2 & e_3 & g_4 & . \\ b_2 & a_3 & c_4 & e_5 & g_6 \\ d_3 & b_4 & a_5 & c_6 & e_7 \\ f_4 & d_5 & b_6 & a_7 & c_8 \\ . & f_6 & d_7 & b_8 & . \end{vmatrix} = c_2 \begin{vmatrix} . & . & e_5 & g_6 \\ d_3 & a_5 & . & e_7 \\ f_4 & b_6 & a_7 & . \\ . & d_7 & b_8 & . \end{vmatrix} + e_3 \begin{vmatrix} . & . & . & . \\ . & b_4 & c_6 & . \\ . & d_5 & a_7 & . \\ . & f_6 & b_8 & . \end{vmatrix}, \quad (2)$$

the first term on the right-hand side giving all the solutions involving c_2 , and the second those involving e_3 . It is clear that if we have obtained the solutions that involve c_2 , we can derive those that involve g_6 , b_8 , and f_4 by merely turning the board through 90° , 180° , and 270° ; while those involving g_4 , c_8 , f_6 , and b_2 are obtained at once by reflexion. The first term of the right-hand member in (2) merely consists of c_2 multiplied by its minor, the constituents involving c and the suffix 2 being replaced by zeros; the third term consists of e_3 and its minor, the constituents involving e and 3, and also g_6 , b_8 , f_4 , c_8 , f_6 , b_2 , being put equal to zero. This term therefore takes the form in (2) and is seen to vanish. The former term

$$= c_2 e_5 \begin{vmatrix} d_3 & . & . \\ f_4 & b_6 & . \\ . & d_7 & b_8 \end{vmatrix} + c_2 g_6 \begin{vmatrix} d_3 & a_5 & . \\ f_4 & . & a_7 \\ . & d_7 & b_8 \end{vmatrix} = c_2 g_6 a_5 f_4 b_8;$$

so that there is only one solution involving c_2 ; and as this only involves c_2, g_6, b_8, f_4 , and a_5 , the middle square on the board, we obtain no fresh solution by the rotation of the board; the only other solution, therefore, is its reflexion, viz. $g_4 b_2 a_5 c_8 f_6$. There are thus ten solutions, which agree (after correcting a couple of misprints) with those found by Dr. Günther by developing the determinant (1) in full.

It is convenient to distinguish the different classes of solutions as ultimate, penultimate, &c.,—the ultimate solutions being those which involve no corner square, the penultimate those which, not being ultimate, involve no square next to a corner square, the antepenultimate those which, not being ultimate or penultimate, involve no square next but one to a corner square, and so on: thus for the board of 25 squares there are 8 ultimate, 2 penultimate, and no antepenultimate solutions. It is also convenient, in considering any class of solutions, to call the constituent which multiplies the minor which is actually developed the leading square; and the three squares which would take its place if the board were turned through 90° , 180° , and 270° vice leading squares, or simply vice squares. Thus, for the board of 25 squares, c_2 is the leading penultimate square, and g_6, b_8, f_4 are the vice squares.

Consider now a board of 36 squares,

$$\begin{vmatrix} a_1 & c_2 & e_3 & g_4 & k_5 & n_6 \\ b_2 & a_3 & c_4 & e_5 & g_6 & k_7 \\ d_3 & b_4 & a_5 & c_6 & e_7 & g_8 \\ f_4 & d_5 & b_6 & a_7 & c_8 & e_9 \\ h_5 & f_6 & d_7 & b_8 & a_9 & c_{10} \\ m_6 & h_7 & f_8 & d_9 & b_{10} & a_{11} \end{vmatrix},$$

it will be noticed that every one of the 10 five-solutions involves an a , so that there is no six-solution involving a_{11} ; there are therefore no ultimate six-solutions. On development we have

$$c_2 \begin{vmatrix} \cdot & \cdot & e_5 & g_6 & k_7 \\ d_3 & a_5 & \cdot & e_7 & g_8 \\ f_4 & b_6 & a_7 & \cdot & e_9 \\ h_5 & d_7 & b_8 & a_9 & \cdot \\ \cdot & f_8 & d_9 & b_{10} & \cdot \end{vmatrix} + e_3 \begin{vmatrix} \cdot & \cdot & \cdot & g_6 & \cdot \\ d_3 & b_4 & c_6 & \cdot & g \\ f_4 & d_5 & a_7 & c & \cdot \\ \cdot & f_6 & b_8 & a_9 & \cdot \\ \cdot & \cdot & d_9 & \cdot & \cdot \end{vmatrix}$$

The first term gives, it will be found, only one penultimate solution involving c_2 , viz. $c_2 e_5 g_8 f_4 d_7 b_{10}$, which involves the vice square b_{10} and is symmetrical: we thus by rotation obtain only one other solution, viz. $b_2 d_5 f_8 g_4 e_7 c_{10}$. These two solutions give by

reflexion $k_5c_4d_3e_9b_8h_7$ and $h_5b_4e_3d_9c_8k_7$; and as there are no antepenultimate solutions, these four penultimate solutions are all that the problem admits of.

Consider now a board of 49 squares,

$$\left| \begin{array}{ccccccc} a_1 & c_2 & e_3 & g_4 & k_5 & n_6 & q_7 \\ b_2 & a_3 & c_4 & e_5 & g_6 & k_7 & n_8 \\ d_3 & b_4 & a_5 & c_6 & e_7 & g_8 & k_9 \\ f_4 & d_5 & b_6 & a_7 & c_8 & e_9 & g_{10} \\ h_5 & f_6 & d_7 & b_8 & a_9 & c_{10} & e_{11} \\ m_6 & h_7 & f_8 & d_9 & b_{10} & a_{11} & c_{12} \\ p_7 & m_8 & h_9 & f_{10} & d_{11} & b_{12} & a_{13} \end{array} \right|, \quad . \quad . \quad . \quad (3)$$

As not one of the 4 six-solutions involves an a , we obtain by merely appending a_{13} to them the 4 seven-solutions that involve a_{13} , viz.

$$\begin{array}{ll} c_2e_5g_8f_4d_7b_{10}a_{13} & b_2d_5f_8g_4e_7c_{10}a_{13} \\ k_5c_4d_3e_9b_8h_7a_{13} & h_5b_4e_3d_9c_8k_7a_{13}. \end{array}$$

These by reflexion give

$$\begin{array}{ll} n_6e_5b_4g_{10}a_9f_8p_7 & n_8e_9b_{10}g_4a_5f_6p_7 \\ e_3g_6k_9d_5b_8a_{11}p_7 & e_{11}g_8k_5d_9b_6a_3p_7; \end{array}$$

and the eight corresponding ultimate solutions that involve the other corner squares a_1 and q_7 are written down at once by replacing each suffix by its complement to 14 in the first four, and by interchanging b and c , d and e , f and g , h and k , m and n , p and q in the last four.

On developing the determinants in the manner explained above, it appears that there are no antepenultimate or preantepenultimate solutions, and that the penultimate solutions which involve c_2 are six in number, viz.

$$c_2n_8e_7b_6h_5a_{11}f_{10}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (i)$$

$$c_2k_7a_5g_{10}b_8m_6d_{11}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (ii)$$

$$c_2e_5d_3g_{10}a_9f_8b_{12}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (iii)$$

$$c_2g_6a_5f_4e_{11}d_9b_{12}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (iv)$$

$$c_2g_6k_9a_7h_5f_8b_{12}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (v)$$

$$c_2g_6d_3a_7e_{11}f_8b_{12}. \quad . \quad . \quad . \quad . \quad . \quad (vi)$$

The vice squares are n_8 , b_{12} , m_6 ; and on turning the board through 90° so that n_8 occupies the position of the leading square c_2 , we obtain five new solutions:

$$n_8 b_{12} a_9 c_6 e_3 h_7 f_4, \quad . \quad . \quad . \quad . \quad . \quad . \quad (vii)$$

$$n_8^e k_5 f_{10} d_7 b_4 m_6, \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{viii})$$

$$n_8 c_{10} e_7 g_4 h_9 d_5 m_6, \quad . \quad . \quad . \quad . \quad . \quad . \quad (ix)$$

$$n_8 c_{10} d_{11} a_7 e_3 b_4 m_6, \quad . \quad . \quad . \quad . \quad . \quad . \quad (x)$$

$$n_8 c_{10} k_5 a_7 h_9 b_4 m_6. \quad . \quad . \quad . \quad . \quad . \quad . \quad (xi)$$

(ii) gives no new solution, as $n_8 a_{11} e_7 f_{10} b_6 c_2 h_5$ which results from it is only a reproduction of (i). A further rotation through 90° , so that b_{12} becomes the leading square, gives only one new solution,

$$b_{12}m_6d_7c_8k_9a_3g_4; \quad . \quad . \quad . \quad . \quad . \quad (xii)$$

and another rotation through 90° gives no new solution. By reflecting (i) ... (xii) we obtain 12 more solutions, so that for the board of 49 squares there are 40 solutions, of which 16 are ultimate and 24 penultimate.

To rotate a solution through 90° in the simplest manner, observe that the corresponding letters on opposite sides of the a diagonal form the pairs b and c , d and e , f and g , h and k , m and n , p and q , so that in any solution to replace each letter by its fellow in its pair (or, say, by its conjugate) is equivalent to turning the board through 90° and then reflecting the result. Thus the process is to (mentally) replace each letter by its conjugate; and (keeping the eye on the representation of the board) to reflect each square, *e. g.* to rotate (i) through 90° , we take from (3) the reflection of b_2 , viz. n_8 ; of m_8 , viz. b_{12} ; of d_7 , viz. a_9 , and so on, thus obtaining (vii). The operations are very readily performed; and it is convenient to always reflect in the manner noted above, viz. by replacing the r th square from one side by the r th square from the other side in the same row. The rotation through 180° is effected by merely replacing each letter by its conjugate, and each suffix by its complement (as in xii, which is so derived from i); and the rotation through 270° is effected by treating similarly the 90° results. Thus (i) turned through 270° gives $m_6c_2a_3b_8d_{11}k_7g_{10}$, which is identical with (ii). It may be noticed that we always obtain a verification of the result thus:—As (i)–(vi) are all the solutions in which c_2 is involved, we see that the vice squares n_8 , b_{12} , m_6 can each only be involved in 6 of the solutions. On counting the number of solutions in which they occur in (i)–(xii), it will be seen that this is the case; and the most certain way of performing the work is to write down all the solutions that result from the rotations through 90° , 180° , and 270° , strike out those that have appeared previously, and then see that in the solutions which remain the leading square and the three vice squares each appear the same number of times.

To obtain from the original and the rotation solutions the corresponding set of reflexion solutions, we might, of course, merely reflect each as it stands; but a more expeditious process is to replace every letter by its conjugate throughout, or every suffix by its complement throughout. By either of these latter methods we get all the reflexions: thus the reflexions answering to (i)–(xii) are either $b_2m_8d_7c_6k_5a_{11}g_{10}$, &c., or $c_{12}n_6e_7b_8h_9a_3f_4$, &c. It may be here remarked that in all the solutions in this paper the constituents are written in the order of the rows or the columns in which they occur: thus in (i) c_2 belongs to the top row, n_8 to the second, and so on; in (vii) n_8 belongs to the right-hand column, b_{12} to the column next it, and so on.

It is convenient to introduce the following definitions:—

An unsymmetrical solution is one which involves no vice square; thus an unsymmetrical solution gives rise to seven more solutions, viz. 3 by rotation and 4 more by reflexion.

A symmetrical solution is one which remains unaltered when the board is turned through 180° .

A quasi-symmetrical solution is one which is not symmetrical, but which involves one or more vice squares.

A pair of conjugate solutions are such that when the board is turned through 180° each reproduces the other; two solutions cannot, therefore, be conjugate unless they are quasi-symmetrical.

Thus (i) and (ii) are quasi-symmetrical, as although they respectively involve the vice squares n_8 and m_6 they are not symmetrical; (iii) and (iv) are conjugate solutions, as the first letter in (iii) and the last in (iv) are conjugates and their suffixes are complementary, the second letter in (iii) and last but one in (iv) are conjugates and their suffixes complementary &c.; (v) and (vi) are symmetrical, as in both of them the first and last letters, the second and last but one letters &c. are conjugates, and the first and last suffixes, the second and last but one, &c. are complementary. The two four-solutions and the two penultimate five-solutions, previously found, are doubly symmetrical.

We now come to the chessboard of 64 squares,

a_1	c_2	e_3	g_4	k_5	n_6	q_7	s_8
b_2	a_3	c_4	e_5	g_6	k_7	n_8	q_9
d_3	b_4	a_5	c_6	e_7	g_8	k_9	n_{10}
f_4	d_5	b_6	a_7	c_8	e_9	g_{10}	k_{11}
h_5	f_6	d_7	b_8	a_9	c_{10}	e_{11}	g_{12}
m_6	h_7	f_8	d_9	b_{10}	a_{11}	c_{12}	e_{13}
p_7	m_8	h_9	f_{10}	d_{11}	b_{12}	a_{13}	c_{14}
r_8	p_9	m_{10}	h_{11}	f_{12}	d_{13}	b_{14}	a_{15}

There were 4 seven-solutions that involved no a ; and these give rise to the following 4 solutions involving a_{15} :

$$\begin{array}{ll} a_{15}n_8e_9k_5f_{10}d_7b_4m_6 & a_{15}m_8d_9h_5g_{10}c_7c_4n_6 \\ a_{15}n_8c_{10}e_7g_4h_9d_5m_6 & a_{15}m_8b_{10}d_7f_4k_9e_5n_6, \end{array}$$

which give on rotation,

$$\begin{array}{ll} r_8a_{13}b_{10}g_{12}d_5c_6k_7e_3 & r_8a_3b_6g_4d_{11}c_{10}k_9e_{13} \\ r_8a_{13}d_9c_{10}k_{11}b_4g_6e_3 & r_8a_3d_7c_6k_5b_{12}g_{10}e_{13}; \end{array}$$

and by replacing every suffix by its complement to 16 in the first four, and every letter by its conjugate in the second four, we obtain the other 8 solutions involving a_1 and s_8 . There are thus 16 ultimate solutions.

On expanding the determinants there are found to be 8 penultimate solutions involving c_2 , viz.

$$\begin{array}{l} c_2e_5g_3k_{11}d_7m_6a_{13}f_{12} \\ c_2g_6k_9f_4d_7e_{13}b_{12}h_{11} \\ c_2g_6k_9a_7h_5e_{13}b_{12}m_{10} \\ c_2k_7d_3g_{10}b_8e_{13}h_9f_{12} \\ c_2k_7n_{10}b_6h_5d_9a_{13}f_{12} \\ c_2n_8e_7k_{11}h_5d_9b_{12}m_{10} \\ c_2n_8a_5e_9g_{12}b_{10}p_7h_{11} \\ c_2q_9g_8f_4d_7b_{10}a_{13}h_{11}, \end{array}$$

six antepenultimate solutions involving e_3 , viz.

$$\begin{array}{l} e_3g_6b_4k_{11}h_5c_{12}f_{10}d_{13} \\ e_3g_6n_{10}a_7h_5c_{12}m_8d_{13} \\ c_3n_8b_4k_{11}a_9m_6f_{10}d_{13} \\ e_3k_7b_4c_8g_{12}m_6a_{13}h_{11} \\ e_3k_7n_{10}f_4a_9c_{12}m_8h_{11} \\ e_3k_7n_{10}d_5b_8m_6a_{13}f_{12}, \end{array}$$

and no preantepenultimate solutions.

Considering first the penultimate solutions, we see that the first six are unsymmetrical and the last two quasi-symmetrical; the former give rise by rotation to 18 additional solutions:

$$\begin{array}{lll} q_9g_{10}a_{11}f_{12}c_6e_3m_8h_5 & b_{14}d_{11}f_8h_5e_9n_{10}a_3g_4 & p_7f_6a_5g_4b_{10}d_{13}n_8k_{11} \\ q_9e_{11}b_{12}k_5c_6m_{10}h_7f_4 & b_{14}f_{10}h_7g_{12}e_9d_3c_4k_5 & p_7d_5c_4h_{11}b_{10}n_6k_9g_{12} \\ q_9e_{11}b_{12}c_8g_4m_{10}h_7d_3 & b_{14}f_{10}h_7a_9k_{11}d_3c_4n_6 & p_7d_5c_4b_8f_{12}n_6k_9e_{13} \\ q_9c_{12}n_6d_{11}a_7m_{10}b_4h_5 & b_{14}h_9e_{13}f_6c_8d_3k_7g_4 & p_7b_4m_{10}e_5a_9n_6c_{12}k_{11} \\ q_9c_{12}d_{13}e_7g_4b_6m_8h_5 & b_{14}h_9m_6c_{10}k_{11}e_7a_3g_4 & p_7b_4e_3d_9f_{12}c_{10}n_8k_{11} \\ q_9a_{13}c_{10}f_{12}g_4b_6h_7d_3 & b_{14}m_8d_9h_5k_{11}e_7c_4n_6 & p_7a_3b_6g_4f_{12}c_{10}k_9e_{13}; \end{array}$$

and the latter to the two,

$$q_9 b_{14} a_{11} k_5 c_6 d_7 m_8 f_4 \quad b_{14} p_7 f_8 g_{12} e_9 c_6 a_3 k_5.$$

Each of these 28 solutions gives rise to another by replacing the letters by their conjugates, or the suffixes by their complements: there are thus altogether 56 penultimate solutions.

Of the antepenultimate solutions involving e_3 as leading square, the first is symmetrical, the second and third are conjugate, and the other three are quasi-symmetrical—two involving one vice square each, and the remaining one two vice squares. By rotation they only give rise to 4 more solutions,

$$\begin{array}{ll} n_{10} e_{11} k_7 f_{12} g_4 h_9 d_5 m_6 & n_{10} c_{12} d_{13} k_5 b_8 h_9 a_3 f_4 \\ n_{10} e_{11} d_{13} c_8 g_4 h_9 a_3 m_6 & d_{13} h_9 m_6 g_{12} a_7 b_4 n_8 k_5. \end{array}$$

Each of these 10 solutions gives rise to another by replacing the letters by their conjugates, or the suffixes by their complements; there are thus altogether 20 antepenultimate solutions. On the whole, therefore, we have 92 solutions, of which 16 are ultimate, 56 penultimate, and 20 antepenultimate. The number 92 is the same as that found by Gauss.

Every unsymmetrical solution, as before remarked, forms one of a group of eight, connected together by rotation or reflexion; and it is natural to inquire how many essentially different solutions the problem admits of. By the essentially different solutions (or, say, the type-solutions or stem-solutions) are meant all the solutions, if we regard a solution and all that can be derived from it by rotation and reflexion merely as one solution. The following will be found to include all the stem-solutions:—

$$\begin{array}{lll} a_{15} n_8 e_9 k_5 f_{10} d_7 b_4 m_6 & c_2 e_5 g_8 k_{11} d_7 m_6 a_{13} f_{12} & e_3 g_6 b_4 k_{11} h_5 c_{12} f_{10} d_{13} \\ a_{15} n_8 c_{10} e_7 g_4 h_9 d_5 m_6 & c_2 g_6 k_9 f_4 d_7 e_{13} b_{12} h_{11} & e_3 g_6 n_{10} a_7 h_5 c_{12} m_8 d_{13} \\ & c_2 g_6 k_9 a_7 h_5 e_{13} b_{12} m_{10} & e_3 k_7 b_4 c_8 g_{12} m_6 a_{13} h_{11}, \\ & c_2 k_7 d_{13} g_{10} b_8 e_{13} h_9 f_{12} & \\ & c_2 k_7 n_{10} b_6 h_5 d_9 a_{13} f_{12} & \\ & c_2 n_8 e_7 k_{11} h_5 d_9 b_{12} m_{10} & \\ & c_2 n_8 a_5 e_9 g_{12} b_{10} p_7 h_{11} & \end{array}$$

the two in the first column being ultimate, the seven in the second penultimate, and the three in the third antepenultimate. There is but one symmetrical solution (viz. the first in the third column); and the number 92 may be accounted for as follows: if none of the twelve stem-solutions were symmetrical there would be $8 \times 12 = 96$ solutions; but one, being symmetrical, gives rise by rotation and reflexion to only three new solutions

(not seven), so that we have a reduction of 4, and the number is $96 - 4 = 92$. As the symmetrical solution is unique, it is perhaps worth giving it here:—

.	1
.	.	.	.	1	.	.
.	1
.	1
1
.	1	.
.	.	1
.	.	.	.	1	.	.

It will be noticed that on the chessboard all the uneven suffixes correspond to white squares, and the even suffixes to black squares; every solution thus involves four white and four black squares.

In developing the determinants I found it most convenient to replace the zero constituents by crosses, and to write down the terms not previously obliterated and scratch them through with the pen—thus, *e. g.*, to write the right-hand side of (2),

$$c_2 \begin{vmatrix} b_2 & c_4 & e_5 & g_6 \\ d_3 & a_5 & c_6 & e_7 \\ f_4 & b_6 & a_7 & c_8 \\ \times & d_7 & b_8 & \times \end{vmatrix} + e_3 \begin{vmatrix} b_2 & a_3 & e_5 & g_6 \\ d_3 & b_4 & c_6 & e_7 \\ f_4 & d_5 & a_7 & c_8 \\ \times & f_6 & b_8 & \times \end{vmatrix}$$

and erase by a stroke the constituents which are printed as points in (2). But a little practice soon indicates the most rapid course of procedure, and suggests several artifices which abbreviate the work. The fact also that the signs of the terms are not required and may all be taken as positive, renders the process of development much less troublesome than it would otherwise be. I performed the whole work twice independently, and found, on comparing the two calculations (which were very different in many respects), that all the results were the same; so that I feel very little doubt of their accuracy. I also verified that every solution contained one square from each row and one from each column.

It is worth while, in conclusion, to place here together the final results for the different boards.

No. of squares.	No. of stem-solutions.	Total number of solutions.
2 ²	0	0
3 ²	0	0
4 ²	1	2
5 ²	2	10
6 ²	1	4
7 ²	6	40
8 ²	12	92

September 17, 1874.

LXII. Notices respecting New Books.

The Correlation of Physical Forces. Sixth Edition. With other Contributions to Science. By the Hon. Sir W. R. GROVE, M.A., F.R.S., one of the Judges of the Court of Common Pleas. London: Longmans, Green, and Co. 1874. (8vo, pp. 466.)

THIS volume contains the sixth edition of Sir W. Grove's well-known essay on the "Correlation of Physical Forces," the third edition of the essay on "Continuity," and a reprint of thirty-eight papers on various scientific subjects which have appeared from time to time in different periodical publications, sixteen of them in our own pages; the most important, however, appeared originally in the Transactions of the Royal Society, viz. those on "The Gas Voltaic Battery," on "The Voltaic Action of Phosphorus, Sulphur, and Hydrocarbons," on "The Effect of Surrounding Media on Voltaic Ignition," on "The Electro-chemical polarity of Gases," and the Bakerian Lecture of A.D. 1846, on "Certain Phenomena of Voltaic Ignition, and the Decomposition of Water into its constituent gases by Heat." These papers, says the author, have not been "altered, further than by correcting some mere verbal errors, and in two or three instances incorporating with the text paragraphs printed at the time as foot-notes." The two essays fill just half the volume; and the five papers whose names we have given above, nearly half the remainder (pp. 112).

The part of the volume best known to the public is, it need hardly be said, the essay on the "Correlation of Physical Forces," originally published in 1846 as the substance of a course of lectures delivered at the London Institution in the year 1843. In a lecture delivered in January 1842, and (apparently) printed shortly after, the leading doctrine of the essay had been enunciated in these terms:—"Physical science treats of Matter, and what I shall to-night term its *Affections*; namely, Attraction, Motion, Heat, Light, Electricity, Magnetism, Chemical Affinity. When these react upon Matter, they constitute Forces. The present tendency of theory seems to lead to the opinion that all these Affections are resolvable into one, namely, Motion. However, should the theories

on these subjects be ultimately so effectually generalized as to become laws, they cannot avoid the necessity for retaining different names for these different affections ; or, as they would then be called, different Modes of Motion . . . Light, Heat, Electricity, Magnetism, Motion, and Chemical Affinity are all convertible material affections ; assuming either as the cause, one of the others will be the effect ; thus heat may be said to produce electricity, electricity to produce heat, magnetism to produce electricity, electricity magnetism ; and so of the rest. Cause and effect, therefore, in their abstract relation to these forces, are words solely of convenience. We are totally unacquainted with the ultimate generating power of each and all of them, and probably shall ever remain so ; we can only ascertain the normæ of their action ; we must humbly refer their causation to one omnipresent influence, and content ourselves with studying their effects and developing by experiment their mutual relations." (Quoted in Preface to fifth edition.) These mutual relations are the subject of an admirable experimental illustration which brings the whole subject under view at a glance, and is thus described :—"A prepared Daguerreotype plate is enclosed in a box filled with water, having a glass front with a shutter over it ; between this glass and the plate is a gridiron of silver wire ; the plate is connected with one extremity of a galvanometer-coil, and the gridiron of wire with one extremity of a Breguet's helix ; the other extremities of the galvanometer and helix are connected by a wire and the needles brought to zero. As soon as a beam of either daylight or the oxyhydrogen light is, by raising the shutter, permitted to impinge on the plate, the needles are deflected ; thus, light being the initiating force, we get *chemical action* on the plate, *electricity* circulating through the wires, *magnetism* in the coil, *heat* in the helix, and *motion* in the needles" (p. 28, 1st edit. ; p. 101, 6th edit.). We may here pause to observe that this view leads to the conclusion that light consists in the vibration of matter itself, and not of a distinct ætherial medium pervading the ordinary forms of matter. This conclusion the author adopts ; and, in fact, he has argued the point both in his lecture and still more fully in his essay ; indeed his words are, "Although this theory has been considered defective by a philosopher of high repute, I cannot see the force of the arguments by which it has been assailed ; and therefore, for the present, though with diffidence, I adhere to it" (p. 109). There can be no doubt that there is great force in the remark that "at the utmost our assumption, on the one hand, is that wherever light, heat, &c. exist, ordinary matter exists, though it may be so attenuated that we cannot recognize it by the test of gravitation ; and that to the expansibility of matter no limit can be assigned. On the other hand, a specific matter without weight must be assumed of the existence of which there is no evidence, but in the phenomena for the explanation of which its existence is supposed. To account for the phenomena the æther is assumed, and to prove the existence of the æther the phenomena are cited. For these reasons, and others

above given, I think that the assumption of the universality of ordinary matter is the least gratuitous" (p. 124). This, however, does not amount to much more than an allegation that this part of the subject is obscure, and that for the present we must acquiesce in the conclusion that, "before this obscurity can be perfectly cleared up, we must know something of the ultimate or *molecular* constitution of the bodies, or groups of molecules, at present known to us only in the aggregate"*.

The present edition differs in many respects from the first, not in substance, but in the way of additional illustrations; *e. g.* on comparing the parts treating of Heat in the two editions, it will be found that the original eight pages exist almost word for word in the new edition, but are expanded into forty-four pages by additions, which serve, for the most part, to mark the progress of science during the last thirty years.

The second essay, on "Continuity," is probably known to most of our readers as the President's Address to the British Association at Nottingham in A.D. 1866; its leading idea is thus enunciated:—"One word will give you the key to what I am about to discourse on; that word is *continuity*—no new word, and used in no new sense, but perhaps applied more generally than it has hitherto been. We shall see, unless I am much mistaken, that the development of observational, experimental, and even deductive knowledge is either attained by steps extremely small and forming really a continuous march, or, when distinct results, apparently separate from any coordinate phenomena, have been attained, that then, by the subsequent progress of science, intermediate links have been discovered uniting the apparently segregated instances with other more familiar phenomena. We shall see that the more we investigate, the more we find that in existing phenomena graduation from the like to the seeming unlike prevails, and in the changes which take place in time gradual progress is, and apparently must be, the course of nature" (p. 186). This idea serves as a thread by which to connect the parts of an interesting survey of the state of science and its recent progress at the time of the composition of the essay; but it is something more than this; it is a just and even a profound conception, though it is not susceptible of so exact a treatment as that of the Correlation of Physical Forces. Thus continuity may (perhaps) be predicated with exactness of the different forms of animal and vegetable life. If we had before us the whole series of past and present existences, and could trace out the tangled meshes of the various lines of their descent, we should probably see all existing gaps filled up and any two given forms connected in some more or less complicated way by a succession of intermediate forms differing almost imperceptibly from each other and derived by descent from some primitive form. Continuity in this sense is plainly something very different from that in which planets, planetoids, and meteorites can be regarded as exemplifying a continuous series. In fact there is no probability that

* Thomson and Tait, 'Treatise on Natural Philosophy,' vol. i. p. 311.

any future discoveries will reveal to us a succession of planets intermediate to the eight major planets; and certainly they present us with any thing but a continuous series of bodies, either in size or density.

The author describes the miscellaneous papers as containing "something of suggestion . . . which may not be altogether useless." He has therefore published them along with his better-known essays as "a last legacy." He tells us, in words which disclose something like regret, of his finding it "necessary to the well-being of others" to relinquish science for law. We may perhaps be allowed to *think* that the interests of the community were well consulted when he devoted to the law powers which his high professional position proves to have found in the law a congenial employment, and at all events to *say* that few men have made science their sole pursuit, who in the evening of life can give so good an account of their occupations as that contained in the present volume. Nor can we forbear to add that Sir W. Grove's eminence in other pursuits gives an almost judicial character to the opinion pronounced in the following words, with which we must end our notice:—"Little can be achieved in scientific research without an acquaintance with it in youth; you will rarely find an instance of a man who has attained any eminence in science who has not commenced its study at a very early period of life. Nothing, again, can tend more to the promotion of science than the exertions of those who have early acquired the *ῥῆθος* resulting from a scientific education. I desire to make no complaint of the tardiness with which science has been received at our public schools, and, with some exceptions, at our Universities. These great establishments have their roots in historical periods; and long time and patient endeavour are requisite before a new branch of thought can be grafted with success on a stem to which it is exotic. Nor should I ever wish to see the study of languages, of history, of all those refined associations which the past has transmitted to us, neglected; but there is room for both. It is sad to see the number of so-called educated men who, travelling by railway, voyaging by steamboat, consulting the almanac for the time of sunrise or full moon, have not the most elementary knowledge of a steam-engine, a barometer, or a quadrant, and who will listen with a half-confessed faith to the most idle predictions as to weather or cometic influences, while they are in a state of crass ignorance as to the cause of the trade-winds or the form of a comet's path. May we hope that the slight infiltration of scientific studies, now happily commenced, will extend till it occupies its fair space in the education of the young, and that those who may be able learnedly to discourse on the Eolic digamma will not be ashamed of knowing the principles on which the action of an air-pump, an electrical machine, or a telescope depends, and will not, as Bacon complained of his contemporaries, despise such knowledge as something mean and mechanical?" (p. 184).

Elementary Dynamics. With numerous Examples. By W. G. WILLSON, M.A., L.C.E., Presidency College. Calcutta: Thacker, Spink, and Co. 1874 (12mo, pp. 278).

Our author tells us that, as regards the “arrangement, method, and demonstrations” of the present work, “much has been derived from Sir W. Thomson and Professor Tait’s ‘Treatise on Natural Philosophy;’ and, indeed, it is plain from the title-page that he adopts their terminology, according to which the science of *Mechanics* with its subdivisions, Statics, and Dynamics, is called *Dynamics* with its subdivisions Statics and Kinetics. Consequently his book is what would be called commonly an Elementary Treatise on Mechanics. That its title is misleading is only an instance of the inconvenience inseparable from the attempt to introduce a new Terminology, even when it has well-founded claims to being an improvement on that which it is designed to replace. There is not much room for novelty in the contents of a volume written on a well-worn subject; thus the part on Statics, which fills half the volume, contains little or nothing that may not be found about equally well done in several elementary books. In other parts of the volume the influence of our author’s guides is more apparent: *e. g.*, he explains the distinction between the gravitation and absolute units of force; he gives the three laws of motion as stated by Newton; he devotes a sufficient part of his volume to work and energy; he separates the subjects of Kinematics and Kinetics. In most, if not all of these points he adopts a course which others have already followed, and which is not likely to be departed from by future writers of books with the same aim as that before us.

The examples are about three hundred and fifty in number, and have been mainly taken from University Examination-Papers. They are well chosen, and the answers seem to be correctly given; at least they are so in the cases in which we have tested them. The text of the work is written clearly and accurately; and though the author does not show any unusual power of elementary exposition, his book will doubtless prove useful to the class which enjoys the benefit of his oral instruction, and probably to others, though we doubt whether it is likely to be much used in England.

LXIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 395.]

March 26, 1874.—Joseph Dalton Hooker, C.B., President, in the Chair.

THE following communication was read:—

“On the Motions of some of the Nebulæ towards or from the Earth.” By William Huggins, D.C.L., LL.D., F.R.S.

The observations on the motions of some of the stars towards

and from the earth which I had the honour to present to the Royal Society in 1872 appeared to show, from the position in the heavens of the approaching and receding stars, as well as from the relative velocities of their approach and recession, that the sun's motion in space could not be regarded as the sole cause of these motions. "There can be little doubt but that in the observed stellar movements we have to do with two other independent motions—namely, a movement common to certain groups of stars, and also a motion peculiar to each star"*.

It presented itself to me as a matter of some importance to endeavour to extend this inquiry to the nebulae, as it seemed possible that some light might be thrown on the cosmical relations of the gaseous nebulae to the stars and to our stellar system by observations of their motions of recession and of approach.

Since the date of the paper to which I have referred, I have availed myself of the nights sufficiently fine (unusually few even for our unfavourable climate) to make observations on this point.

The inquiry was found to be one of great difficulty, from the faintness of the objects and the very minute alteration in position in the spectrum which had to be observed.

At first the inquiry appeared hopeless, from the circumstance that the brightest line in the nebular spectrum is not sufficiently coincident in character and position with the brightest line in the spectrum of nitrogen to permit this line to be used as a fiducial line of comparison. The line in the spectrum of the nebulae is narrow and defined, while the line of nitrogen is double, and each component is nebulous and broader than the line of the nebulae. The nebular line is apparently coincident with the middle of the less refrangible line of the double line of nitrogen†.

The third and fourth lines of the nebular spectrum are undoubtedly those of hydrogen; but their great faintness makes it impossible to use them as lines of comparison under the necessary conditions of great dispersive power, except in the case of the brightest nebulae.

The second line, as I showed in the paper to which I have referred, is sensibly coincident with an iron line, wave-length 495.7; but this line is inconveniently faint, except in the brightest nebulae.

In the course of some other experiments my attention was directed to a line in the spectrum of lead which falls upon the less refrangible of the components of the double line of nitrogen. This line appeared to meet the requirements of the case, as it is narrow, of a width corresponding to the slit, defined at both edges, and in the position in the spectrum of the brightest of the lines of the nebulae.

In December 1872 I compared this line directly with the first line in the spectrum of the Great Nebula in Orion. I was delighted to find this line sufficiently coincident in position to serve as a fiducial line of comparison.

* Proceedings of the Royal Society, vol. xx. p. 392.

† Proceedings of the Royal Society, vol. xx. p. 380.

I am not prepared to say that the coincidence is perfect; on the contrary, I believe that if greater prism-power could be brought to bear upon the nebulae, the line in the lead spectrum would be found to be in a small degree more refrangible than the line in the nebulae.

The spectroscope employed in these observations contains two compound prisms, each giving a dispersion of $9^{\circ} 6'$ from A to H. A magnifying-power of 16 diameters was used.

In the simultaneous observation of the two lines it was found that if the lead line was made rather less bright than the nebular line, the small excess of apparent breadth of this latter line, from its greater brightness, appeared to overlap the lead line to a very small amount on its less refrangible side, so that the more refrangible sides of the two lines appeared to be in a straight line across the spectrum. This line could be therefore conveniently employed as a fiducial line in the observations I had in view.

In my own map of the spectrum of lead this line is not given. In Thalén's map (1868) the line is represented by a short line to show that, under the conditions of spark under which Thalén observed, this line was emitted by those portions only of the vapour of lead which are close to the electrodes.

I find that by alterations of the character of the spark this line becomes long, and reaches from electrode to electrode. As some of those conditions (such as the absence of the Leyden jars, or the close approximation of the electrodes when the Leyden jars are in circuit) are those in which the lines of nitrogen of the air in which the spark is taken are faint or absent, the circumstance of the line becoming bright and long or faint and short, inversely as the line of nitrogen, suggested to me the possibility that the line might be due not to the vapour of lead, but to some combination of nitrogen under the presence of lead vapour. As, however, this line is bright under similar conditions when the spark is taken in a current of hydrogen, this supposition cannot be correct.

A condition of the spark may be obtained in which the strongest lines of the ordinary lead spectrum are scarcely visible, and the line under consideration becomes the strongest in the spectrum, with the exception of the bright line in the extreme violet.

I need scarcely remark that the circumstance of making use of this line for the purpose of a standard line of comparison is not to be taken as affording any evidence in favour of the existence of lead in the nebulae.

Each nebula was observed on several nights, so that the whole observing time of the past year was devoted to this inquiry. In no instance was any change of relative position of the nebular line and the lead line detected.

It follows that none of the nebulae observed shows a motion of translation so great as 25 miles per second, including the earth's motion at the time. This motion must be considered in the results to be drawn from the observations; for if the earth's motion

be, say, 10 miles per second from the nebula, then the nebula would not be receding with a velocity greater than 15 miles per second; but the nebula might be approaching with velocity as great as 35 miles per second, because 10 miles of this velocity would be destroyed by the earth's motion in the contrary direction.

The observations seem to show that the gaseous nebulae as a class have not proper motions so great as the bright stars. It may be remarked that two other kinds of motion may exist in the nebulae, and, if sufficiently rapid, may be detected by the spectroscope:—1. A motion of rotation in the planetary nebulae, which might be discovered by placing the slit of the instrument on opposite limbs of the nebulae. 2. A motion of translation in the visual direction of some portions of the nebulous matter within the nebula, which might be found by comparing the different parts of a large and bright nebula.

Sir William Herschel states that “nebulae were generally detected in certain directions rather than in others, that the spaces preceding them were generally quite deprived of stars, that the nebulae appeared some time after among stars of a certain considerable size and but seldom among very small stars, that when I came to one nebula I found several more in the same neighbourhood, and afterwards a considerable time passed before I came to another parcel”*.

Since the existence of real nebulae has been established by the use of the spectroscope, Mr. Proctor† and Professor D'Arrest‡ have called attention to the relation of position which the gaseous nebulae hold to the Milky Way and the sidereal system.

It was with the hope of adding to our information on this point that these observations of the motions of the nebulae were undertaken.

In the following list the numbers are taken from Sir J. Herschel's ‘General Catalogue of Nebulae.’ The earth's motion given is the mean of the motions of the different days of observation.

No.	h.	H.	Others.	Earth's motion from Nebula.
1179	360	..	M. 42	7 miles per second.
4234	1970	..	Σ. 5	12 “ “
4373	..	IV. 37.	..	1 “ “
4390	2000	..	Σ. 6	2 “ “
4447	2023	..	M. 57	3 “ “
4510	2047	IV. 51.	..	14 “ “
4964	2241	IV. 18.	..	13 “ “

* Philosophical Transactions, 1784, p. 448.

† Other Worlds than Ours, pp. 280-290.

‡ Astronomische Nachrichten, No. 1908, p. 190.

LXIV. *Intelligence and Miscellaneous Articles.*

ON THE INTENSITY OF THE LIGHT REFLECTED FROM GLASS.

BY DR. P. GLAN.

HITHERTO the investigations of the properties of reflected light have been, by preference, occupied with the ratio of the two principal components to each other, as well in respect of the phase as of the intensity; and only a few works have attempted a direct determination of the two with respect to the incident light. The only series of experiments on the intensity, which are found in Arago's posthumous Works, for the white light reflected by a plane-parallel glass plate, leaves out of consideration the absorption of the glass; and then it does not give the index of refraction or the angle of polarization; so that the results cannot well be compared with the theory. On that account I have here undertaken again this determination, in order before all things to test the correctness of Cauchy's formula (coinciding with Fresnel's) for light polarized parallel to the plane of incidence. The observations could be limited to the light polarized parallel to the incidence-plane, since from numerous experiments the ratio between the two principal components is known, and so with the determination of the first the second is also given.

The photometric method which I employed is based upon the comparison of the brightness of two adjacent equally illuminated fields, the intensity of which could be altered in a proportion known from the construction of the aggregate. For this purpose a doubly refracting prism was fixed to the collimator-lens of a theodolite so that its principal section was parallel to the slit. The objective of the observing-telescope carried a Nicol with a division-circle, which gave readings to minutes; and the spectral analysis of the light was effected by the set of prisms of a Hofmann spectroscope fixed upon the table of the apparatus. The slit was divided into two parts by a strip of tinfoil, whose breadth was such that the ordinary image of the one half and the extraordinary image of the other were in exact contact; and consequently two adjacent spectra were obtained, the intensities of which, with equal brightness of the two halves of the slit, were to each other as $k \sin^2 \alpha$ to $h \cos^2 \alpha$, if α denotes the angle which the polarization-plane of the Nicol makes with the principal section of the doubly refracting prism, and h and k the coefficients of enfeeblement conditioned by the passage through the apparatus of the two rays polarized perpendicular to one another.

In consequence of the dispersion of the doubly refracting prism, perfect contact is only possible for one colour; but the strong dispersion of the prisms of the Hofmann spectroscope makes that place sufficiently wide for the maximum of sensitiveness to be obtained for the colour in question. A slight inclination of the doubly refracting prism about an axis perpendicular to its principal section

suffices to produce close contact at any part of the spectrum the observation of which is desired.

For the arrangement of the apparatus, the Nicol without the insertion of the prisms was turned so that one image of the slit vanished; then the prisms were prefixed and rotated until again only one image of the slit was to be seen. Their incidence-plane then coincides with the principal section of the doubly refracting prism; and this gives at the same time the point from which the rotation of the Nicol is to be reckoned. In front of the lower half of the slit a rectangular glass prism was fixed, which reflected the light of a laterally placed petroleum-flame into the apparatus. On a level with the upper half stood the telescopes of a small spectrometer provided with a little turntable, by which the light of a second flame was concentrated upon the upper half of the slit. The tube of the collimator, standing next to the flame, had, in place of the slit, a circular aperture; and the two telescopes, provided with only the objective-lenses, were placed so that the lens of the second threw upon the upper half of the slit of the photometer a sharp image of the aperture, which was in the focus of the first.

For the observation the telescope next the flame was placed at 180° , and its objective lens replaced by a Nicol prism arranged so as to cause the disappearance of the lower image of the upper half of the slit. The tube, with the flame in front of it, was then rotated 40° , the glass prism to be investigated fastened lightly with wax upon the table so that it reflected the light of the flame into the apparatus, and inclined until the lower extraordinary image disappeared. The incidence-plane was then perpendicular to the principal section; and as the extra image of the upper half of the slit was compared with the ordinary one of the lower, the observations hold good for light polarized parallel to the plane of incidence.

For observation under various incidences, the two conterminous spectra were brought to equal brightness by direct light; then the collimator-tube, with the flame, was rotated double the desired angle of incidence, and the table with the glass prism so placed that the reflected image of the aperture appeared again at the same spot on the photometer as the direct image, indicated by a mark. If α and β are the angles made by the polarization-plane of the Nicol (with equal brightness in both cases) with the principal section of the doubly refracting prism, then the intensity of the reflected light, in parts of the incident, is

$$R = \frac{\tan^2 \beta}{\tan^2 \alpha}.$$

In order to make myself independent of accidental alterations in the brightness of the two flames, five successive experiments were made each time, in direct and reflected light, for the same angle of incidence; and in the calculation each three consecutive ones were combined. The whole of the observations are valid for the place

in the spectrum corresponding to the green light of thallium. They give the ratio of the intensity of the reflected to the incident light for two prisms—one of crown, and one of flint glass—the non-reflecting surfaces of which were blackened with soot. The results, each the mean of twelve experiments, are contained in the following Table :—

i.	R.	
	Crown glass.	Flint glass.
30°	0·055	0·070
40	0·072	0·084
50	0·104	0·120
55	0·133	0·161
60	0·174	0·203
65	0·231	0·254
70	0·293	0·327

For a comparison with Fresnel's formula a more exact determination of the indices of refraction is needed. Their values as determined from the deflections by the prisms cannot be used for this purpose, since Seebeck has shown that the density, and therewith also the exponent of refraction of the glass-surface, may be considerably altered by the grinding; hence the calculation is based on the values obtained from the tangent of the polarization-angle. This and the principal azimuth were determined for both prisms by aid of the Babinet compensator, and gave the values stated under i_1 and A in the next Table; n is the value found for the line E from the minimum of deflection.

Crown Glass.

$$i_1 = 56^\circ 25' \cdot 5; \quad n' = 1 \cdot 507.$$

$$A = 1^\circ 20'.$$

$$n = 1 \cdot 5275.$$

i.	Observed.	Calculated.	Δ .
30°	0·055	0·059	—0·004
40	0·072	0·073	—0·001
50	0·104	0·114	—0·010
55	0·133	0·141	—0·008
60	0·174	0·179	—0·005
65	0·231	0·229	+0·002
70	0·293	0·302	—0·009

Flint Glass.

$$i_1 = 57^\circ 37'5; \quad n' = 1.577.$$

$$A = 2^\circ 31'5.$$

$$n = 1.6218.$$

i .	Observed.	Calculated.	Δ .
30°	0.070	0.071	-0.001
40	0.084	0.093	-0.009
50	0.120	0.133	-0.013
55	0.161	0.162	-0.001
60	0.203	0.203	0.000
65	0.254	0.257	-0.003
70	0.327	0.330	-0.003

The differences between the intensity calculated from Fresnel's formula for light polarized parallel to the plane of incidence and that directly observed fall quite within the range of the errors of observation; for the maxima and minima of the individual observations diverge from one another, on the average, 0.04.—*Monatsbericht der kön. preuss. Akademie der Wissensch. zu Berlin*, July 1874, pp. 511-516.

POLARIZATION OF THE PLATES OF CONDENSERS. BY A. S. THAYER.

It is well known that in polarization-batteries, of which Planté's battery is a type, a combination of the ions resulting from electrolysis takes place when the plates of the battery are connected, and a current results which slowly diminishes in strength. In the case of condensers made with solid dielectrics the same diminishing current is observed; and the following experiments would seem to show that it might be due to an electrolysis or decomposition of the material separating the plates of tinfoil. The experiments consisted in placing condensers of various kinds in a circuit, through which a current was made to pass by two Bunsen's cells, and noting their changes. The plates of the condensers were of tinfoil, and had an area of about fifteen square inches. The experiments were as follows:—

(1) The dielectric used was a sheet of dry glazed paper. The condenser could not be charged so as to give a perceptible discharge.

(2) When a sheet of glazed paper, moistened with shellac, was substituted for the dry paper, the discharge was sufficient to send the light off the scale of the galvanometer, and continued for some minutes.

(3) Dry goldbeaters' skin was used as a dielectric; and no deflection could be obtained.

(4) The goldbeaters' skin, when moistened with shellac, gave a slowly diminishing deflection.

(5) The dielectric was made by flowing the surfaces of the plates with a solution of wax and gasoline; and a slowly diminishing deflection was obtained.

(6) The condenser used in (4) was tried again after the shellac had dried, and again gave a diminished deflection, less than the first deflection.

(7) The condenser used in (5), when tried again after a day or two, did not again give a deflection.

(8) Unglazed paper dry and oiled gave no deflection.

(9) Glazed paper oiled gave a very slight deflection, and the galvanometer-needle immediately returned to zero.

(10) Glazed paper wet with water and covered with shellac gave the greatest deflection of all the dielectrics. The light was sent completely off the scale, and was only brought back by shunting the galvanometer. The discharge also continued a long time.

(11) The conducting-power of some of the various dielectrics was tested. The goldbeaters' skin which had been covered with shellac transmitted no current after it had been allowed to stand for a week. Freshly oiled and dry oiled paper did not conduct at all. Glazed paper covered with shellac gave a deflection nearly off the scale. Glazed paper wet with water and covered with shellac transmitted a current sufficient to send the light entirely off the scale.

What these experiments directly go to show are, first, that condensers with moist dielectrics received a greater charge than those made with dry, and, second, that the better the dielectric conducted, the greater the charge the condenser was capable of receiving. From these facts it would seem that the slow discharge of these condensers was very probably due to polarization. The best condensers, as shown by the experiments, possessed dielectrics which were moist and possessed considerable conductivity. The dielectrics when dry scarcely conduct at all. Their conduction when moist must therefore have been mainly due to electrolysis, since liquids conduct electricity only in very small quantities without being decomposed. The electrolyte was therefore decomposed; and the recombination of the products of decomposition caused the return current. An exact analogy is thus determined between the case of the lead plates and these condensers. Whether it is an analogy that would hold in the case of all condensers which slowly discharge themselves, is an interesting question.—Silliman's *American Journal*, September 1874.

ON ELECTRICAL CURRENTS ACCOMPANYING THE NON-SIMULTANEOUS IMMERSION OF TWO MERCURY ELECTRODES IN VARIOUS LIQUIDS. BY G. QUINCKE.

[The author, after a very full description in detail of a great number of experiments (Pogg. *Ann.* vol. cliii. pp. 161-203), sums up the results as follows.]

1. If two mercury electrodes, connected by the wire of a multiplier, be immersed one after the other in any liquid which is a conductor of electricity (water, alcohol, glycerine, saline solutions, hydrochloric acid, &c.), an electric current is observed, which goes from the freshly wetted mercury-surface through the liquid to the mercury-surface which has been wetted longest.

2. The intensity of this current diminishes as the resistance of the liquid column between the electrodes is increased.

3. The electromotive force of the current varies according to the nature and the concentration of the liquid, decreases as the concentration of the saline solution increases, and may amount to 0.6 of the electromotive force of a Daniell's element.

4. The electromotive force is as much greater as the boundary surface of mercury with the surrounding liquid on the later-immersed electrode is more quickly produced. With increasing velocity of production of the boundary surface the electromotive force approaches a maximum, which in the case of viscous fluids, like glycerine, is very soon reached.

5. The electromotive force stands in no relation to the quantity of the capillary constant of the common boundary surface of mercury and the surrounding liquid.

6. The reason of these electrical currents is probably to be sought in the alteration of the molecular condition (change of density or concentration) which is gradually accomplished, in the particles of the liquid in the vicinity of the surface of contact with the mercury, after the wetting.

7. Upon non-simultaneous wetting of solid metals by water and other liquids, electrical currents ensue in like manner, and for the same reason, as upon the non-simultaneous wetting of mercury.

8. The electrical currents generated by the non-simultaneous immersion of mercury electrodes in sulphuric acid, nitric acid, &c. have their cause chiefly in the substances formed by chemical action on the mercury; they are therefore secondary phenomena, or chemical-polarization currents.

9. As has long been known, the surface-tension of the common boundary of mercury with other liquid conductors of electricity may be altered by electrolysis.

10. This alteration may consist in an augmentation or a diminution, and may change its sign with the direction and duration of the electric current.

11. The disturbances which occur in capillarity-phenomena cannot be accounted for by substances electrolytically separated.

12. Since accidental and unavoidable impurities considerably modify the amount of the surface-tension of mercury and other liquids, it is not advisable to determine from that amount the quantity of an electrolytically formed substance, or even to deduce from it indirectly the intensity of electric currents or electromotive forces.

—Poggendorff's *Annalen*, vol. cliii. pp. 203-205.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

SUPPLEMENT TO VOL. XLVIII. FOURTH SERIES.

LXV. *The Heat-conducting Power of Mercury independent of the Temperature.* By HERMANN HERWIG*.

ACCORDING to a proposition enunciated by Wiedemann and Franz†, the metals have an approximately equal conducting-power for electricity and heat. Now it would be of the greatest importance to know if the correspondence between the two conducting-powers is *perfect*, and especially whether it subsists *under all temperatures*. If we wish to institute investigations in this direction, we must, in regard to their practicability, distinguish two things. The first portion of the problem, to demonstrate the exact equality of the two powers of any metal at a determined temperature, is attended with the great difficulty of obtaining metals perfectly homogeneous and, above all, unaltered by the experiments themselves. On the other hand, the influence of temperature on the conducting-powers offers far more favourable conditions for an investigation. From numerous experiments, especially those made by Arndtsen‡, and by Matthiessen and Von Bose§, it is known that, for the electric conductivity, a very marked variability with the temperature takes place, and indeed, for *by far the greater number of metals*, in *not very different degrees*. There must, accordingly, if the correspondence made known by Wiedemann and Franz were really so widely valid, be found also for the heat-conducting power of most metals a variability with temperature in about the same degree; and therein not only somewhat different qualities of the same metal, but, as has been said, quite different metals would be fairly equivalent. This side of the question is therefore very suitable for investigation, and it also appears to me to touch most deeply the essence of the entire question.

* Translated from a separate impression, communicated by the Author, from Poggenдорff's *Annalen*, vol. cli. pp. 177-194.

† Pogg. *Ann.* vol. lxxxix. p. 531.

‡ *Ibid.* vol. civ. p. 56.

§ *Ibid.* vol. cxv. p. 389. *Phil. Mag.* S. 4. vol. xxiv. p. 465.

Phil. Mag. S. 4. No. 321. *Suppl.* Vol. 48.

And yet, hitherto, in this direction very few statements have been made, and, one may say, those scarcely in harmony with one another. While some of the older observers hold that the heat-conducting power is in general constant, Wiedemann and Franz appear to admit for copper none, for Rose's metal a slight variability of it with the temperature. Ångström* gives for copper a variability amounting to less than half, and for iron to fully one half of the value of that which holds for electric conduction. Forbes† obtained for iron a still less variability than Ångström.

In face of these various statements, M. Lorenz‡ has recently asserted the independence of temperature of the conductivity for heat of pure metals which remain homogeneous, and accounted for the observed variabilities by the production of thermoelectric currents in consequence of unequal heating of the metals. In fact, iron (for which chiefly, according to the above account, a variable power of conduction is found) is shown by the experiments of Le Roux§ to be peculiarly favourable for such thermoelectric actions.

Accordingly it seems to me that it is still an open question whether, and to what degree, the heat-conducting power of metals depends on their temperature, and that, up to the present time, two simple solutions of it have been proposed. These solutions would be contained in Wiedemann and Franz's law, supposed to be generally valid, that the heat-conducting power is *just as variable* as the electric conduction-power, and, on the other hand, in Lorenz's law, according to which the former power is *invariable* for all temperatures.

But if the position of the question be viewed thus, it is absolutely necessary, for its decision, to submit to investigation a *pure metal which remains homogeneous*. And of this kind, perhaps, only mercury is known. Mercury can be obtained purer than any other metal; and no variations of internal structure can be admitted to take place in it in consequence of partial heating. The latter is excluded by the experiments of Magnus||, according to which warm and cold mercury brought into contact never give rise to electromotive action. Mercury would, accordingly, be the most suitable metal for this investigation. On the other hand, there are two circumstances opposed to the choice of mercury—first, that the variability of its galvanic conductivity is exceptionally less than that of the rest of the metals, and, secondly, the fluidity of this metal. The former circumstance

* Pogg. *Ann.* vol. cxviii. p. 429. † Edinb. *Phil. Trans.* vol. xxiv.

‡ Pogg. *Ann.* vol. cxlvii. p. 435; *Phil. Mag. S. 4.* vol. xlv. p. 65.

§ *Ann. de Chim. et de Phys. Sér. 4.* vol. x.

|| Pogg. *Ann.* vol. lxxxiii. p. 495.

is certainly not desirable; nevertheless even mercury possesses a galvanic conductivity the variability of which is always very apparent and sufficient for the intended investigation. And, as regards the second, I do not think that disturbing currents are to be apprehended if a rather *narrow* mercury-tube be taken and, of course, heated from *above* in a vertical position. Even with tubes of 37 millims. diameter, Ångström* obtained normal results for the heat-conduction of mercury. So much the more could I hope to see no disturbances enter in the tubes employed by me, which were about 1 centim. in diameter.

§ 2.

It was, then, my intention, with such mercury-tubes, to investigate simultaneously electrical conduction and the conduction of heat in their dependence on temperature. I give, in the following, first, the general principle of the method, which is calculated, by one and the same experiment, to bring into the expression the variability of the conducting-power as well for heat as for electricity.

Let us imagine a metallic rod heated (or a pretty narrow tube filled with mercury heated from above), and at the same time a powerful current passed through it. After the stationary condition has come in, the quantities of heat generated in any cross section by the double action of the heat-conduction and the current must be continually given up again outwards. According to this, the following differential equation is formed. Let the cross section q be at the distance x from the initial point and have the temperature t while the temperature of the surrounding air is θ . Let the circumference of the cross section be p , the outer heat-conducting capacity h , the current-intensity i . If, then, R is the galvanic resistance for 0° , I put for the resistance at t° $R(1 + \beta t)$. This form of the variability of R with the temperature is quite sufficient for our present purpose; and moreover, for mercury especially, it is in harmony with the experiments of most experimenters.

For the heat-conduction I likewise introduce the specific resistance at 0° , and put that which holds for t° equal to $r(1 + \alpha t)$. Then, for the unit of time,

$$\frac{q}{r(1 + \alpha t)} \cdot \frac{d^2 t}{dx^2} + \frac{\text{const } i^2}{q} R(1 + \beta t) = ph(t - \theta).$$

If therein we put

$$A = \frac{\text{const } i^2 R r}{q^2} \quad \text{and} \quad B = \frac{p h r}{q},$$

* Pogg. Ann. vol. cxxiii p. 638.

we obtain

$$\frac{d^2t}{dx^2} = -A(1+\alpha t)(1+\beta t) + B(t-\theta)(1+\alpha t).$$

From this differential equation, only the first differential quotient is, in the first place, derived, viz.

$$\frac{dt}{dx} = -\sqrt{2B} \sqrt{-\frac{A}{B} \cdot t - \frac{A}{B} \cdot \frac{\alpha+\beta}{2} t^2 - \frac{A}{B} \cdot \frac{\alpha\beta}{3} t^3 + \frac{t^2}{2} + \frac{\alpha t^3}{3} - \theta t - \frac{\alpha\theta t^2}{2} + \text{const.}}$$

The integration-constant here occurring is determined from the circumstances that for the end of the rod, where alone a current-action is now to be perceived, a temperature t' no longer variable prevails; therefore

$$\text{const} = \frac{A}{B} t' + \frac{A}{B} \cdot \frac{\alpha+\beta}{2} t'^2 + \frac{A}{B} \cdot \frac{\alpha\beta}{3} t'^3 - \frac{t'^2}{2} - \frac{\alpha t'^3}{3} + \theta t' + \frac{\alpha\theta t'^2}{2}.$$

For the further consideration the heat developed, in the unit of time, in the *entire* rod is now calculated, and that given out put equal to it. This leads to an equation which is to be conceived as an infinite sum of equations of the preceding kind. If X is the length of the rod, and T the initial temperature, then the equation is

$$\frac{-q}{r(1+\alpha T)} \left(\frac{dt}{dx} \right)_T \int_0^X \frac{Aq}{r} (1+\beta t) dx = \int_0^X \frac{Bq}{r} (t-\theta) dx.$$

The value $\left(\frac{dt}{dx} \right)_T$ is the previously determined differential quotient $\frac{dt}{dx}$ for the temperature T , while the integration-constant of course possesses the value stated. The two other terms of the present equation contain as a single complicated constituent the integral $\int_0^X t dx$. This integral, however, is simply equal to the *mean temperature* of the entire rod multiplied by the length of the latter. Naming the mean temperature m , the equation therefore becomes

$$\begin{aligned} & \frac{\sqrt{2B}}{1+\alpha T} \sqrt{-\frac{A}{B} (T-t') - \frac{A}{B} \cdot \frac{\alpha+\beta}{2} (T^2-t'^2) - \frac{A}{B} \cdot \frac{\alpha\beta}{3} (T^3-t'^3)} \\ & \quad + \frac{T^2-t'^2}{2} + \frac{\alpha}{3} (T^3-t'^3) - \theta(T-t') - \frac{\alpha\theta}{2} (T^2-t'^2) \\ & = BX \left\{ -\theta - \frac{A}{B} + m \left(1 - \frac{A}{B} \beta \right) \right\}. \end{aligned}$$

Herein $\frac{A}{B}$ is still to be determined, which is done as follows.

In any cross section near the end of the rod, where only current-action takes place, we have

$$\frac{\text{const } i^2}{q} R(1 + \beta t') = ph(t' - \theta),$$

or

$$\frac{Aq}{r} (1 + \beta t') = \frac{Bq}{r} (t' - \theta),$$

therefore

$$\frac{A}{B} = \frac{t' - \theta}{1 + \beta t'}.$$

If this value be inserted and the above equation be given a simpler form, it is readily converted into

$$3BX^2 = \frac{1 + \beta t'}{1 + \beta \theta} \left(\frac{T - t'}{m - t'} \right)^2 \cdot \frac{3 + 2\alpha T + \alpha t'}{(1 + \alpha T)^2}.$$

In accordance with this formula, therefore, we have in the experiment, besides the surrounding temperature, only the three temperatures T , t' , and m to determine and thus obtain a number of equations between α and β .

§ 3.

To this purpose I now directed the following apparatus for the investigation of mercury. On the proper experimental tube (filled with mercury which had been during several months treated with protonitrate of mercury and thereby rendered very pure), of about 1 centim. diameter, and nearly 60 centims. length, and very carefully selected as accurately cylindrical, only the temperatures T and m were to be determined. A mercury thermometer extending to 200°C. , and with a very small cylindrical bulb, served for the determination of T . It was ground into a small glass lateral appendix borne by the tube immediately below its upper extremity, and with its bulb occupied only a small portion of the tube's diameter. The indications of this thermometer of course correspond to the mean temperature of the mercury in its bulb. Now, since this mercury itself, and indeed perpendicular to the axis of the cylinder of the bulb of the thermometer, took part in the heat-conduction, the mean temperature indicated existed in a cross section through the thermometer-bulb and the experimental tube the course of which ran in the upper half of the bulb. If, then, in the above calculation we put for the upper temperature of the entire mercury-tube that indicated by the thermometer, we have had in reality, with

respect to the calculation, on the one hand as much too little mercury in play as was pressed out by the larger lower half of the bulb, and, on the other hand, that quantity of mercury too much which upwards from the cross section was still present. If we now try to equalize as nearly as possible these two quantities in respect of their difference of temperature, we can entirely eliminate the inaccuracy hence arising. For this purpose I have for the average proportions of my experiments supposed the above-mentioned cross section at about one third of the height of the thermometer-bulb from above, and fitted and brought near the firmly cemented iron end of the experimental tube (the bulb was, besides, nowhere in contact with iron, but everywhere with mercury; it was, however, in its upper parts with suitable closeness surrounded by the iron) so that the equalization was as nearly as possible attained. I have taken the greatest error which could be made in this equalization, and with it calculated that even in extreme cases in experiment the effect upon the determination of the temperatures m (which are to be presently discussed) would amount to less than a hundredth part of a degree. Consequently the indications of the thermometer can indeed be regarded as the upper temperatures of the mercury-tube in the sense of the calculation.

Now, before the commencement of the experiments this thermometer was carefully and in detail compared in an oil-bath with an air-thermometer, and, indeed, since it would subsequently always be made use of with a protruding thread, likewise in this position. At the close of the experiments I again controlled the thermometer, and found it unaltered. I had moreover, before using the thermometer, repeatedly and continuously heated it up to its highest temperature in order to give it a definitive state. The temperatures which are subsequently given according to this thermometer are, as has been said, directly temperatures of the air-thermometer. As the thermometer was graduated only to whole degrees, the reading could be accomplished to about $0^{\circ}2$.

The other temperature to be determined on the experimental tube itself was the mean temperature m . For its determination, a mercury-tube could be arranged either as an outflow or as an ordinary thermometer. At the commencement I preferred the former. To this end a very fine outflow-aperture was made in the upper iron closure of the tube, which permitted the mercury to issue in very minute drops. It was further requisite to determine the capacity of the tube at 0° and the coefficient of expansion of the glass. This latter was shown by a number of measurements to be 0.000028 cubic per degree Celsius, therefore somewhat greater than usual. With these data and the

accurately known coefficients of expansion of mercury for every temperature, the temperatures m could be calculated from the quantities which flowed out.

Finally, a piece of identical tube shut off by itself, of about 20 centims. length, which was likewise arranged as an outflow-thermometer and extended below the main tube in direct continuation with it, was to serve for the determination of the lower temperature t' of the tube, which only in consequence of a current rose above the surrounding temperature. In order to cause this piece to be heated in like manner by the current, the latter had, immediately after passing through it, to enter a third piece of tube, which possessed an equal width and a like closure of iron as the two others. This lowest tube stood on a long, screwed ivory point; and all three were held each by a wide ring into which three long points of ivory were screwed as radii—the uppermost, proper experiment-tube at its lower end. Thus no diversion whatever of the current or heat could take place.

I must yet go briefly into the manner of heating, which had to be effected from above. The upper iron closure of the proper experimental tube had been made of somewhat greater diameter outside of the tube, and possessed an upward-directed rim. The vessel hereby formed was filled with mercury; and into this dipped a large conical-bottomed iron reservoir, which was crammed full of brass refuse from the turning-lathe. A circle of gas-jets was then brought round the middle of the reservoir. With this arrangement, low as well as high temperatures T could be maintained in the tube with extraordinary constancy. In order to prevent downward radiation from the (especially in higher temperatures) strongly heated iron reservoir, underneath the burners a screen of sheet brass closely surrounded the reservoir, and below this were three of stout pasteboard, all separated from one another by strata of air. Between the lower screens, also, ran several coils of a leaden pipe, through which cold water was incessantly flowing. Lastly, the small projecting part of the iron closure of the tube was itself underlaid with loose asbestos. The effect of all these precautions was entirely satisfactory; indeed the entire length of the experiment-tube was found to have the same cool environment. That this surrounding temperature might not be disturbed by the observer himself, the entire apparatus was surrounded, from the screens downwards, with glass windows. The experiments were performed in a cellar of the laboratory, which was not exposed to rapid vicissitudes of temperature.

With this apparatus I obtained the results set down in the following Tables—first for heat-conduction alone, *without a simultaneous passage of a current*, so that t' and θ were equal.

At the same time the initial temperatures T taken were not lower than about 40° , because the determinations always become less sure with too little difference between m and t' .

TABLE I.

The volume of the tube at 0° obtained with this filling was 62.2881 cubic centims.

Experiment.	T .	t' .	m .	$\frac{T-t'}{m-t'}$.
1.	39.15	13	15.05	12.76
2.	52.35	13.2	16.14	13.32
3.	72.65	14.4	18.93	12.86
4.	92.95	15.25	21.27	12.91
5.	111.6	15.45	23.07	12.63
6.	139.05	16.3	26.05	12.59
7.	174.7	15.55	27.88	12.91

TABLE II.

The volume of the tube at 0° found, 62.2879 cubic centims.

Experiment.	T .	t' .	m .	$\frac{T-t'}{m-t'}$.
8.	38.65	15.3	16.99	13.81
9.	51.75	15.2	17.83	13.89
10.	87.95	15.3	20.46	14.08
11.	110.2	15.6	22.7	13.32
12.	135.15	16.05	25.27	12.92
13.	161.03	16.3	27.48	12.95
14.	178.9	16.5	28.91	13.08

TABLE III.

The volume of the tube at 0° found, 62.2895 cubic centims.

Experiment.	T .	t' .	m .	$\frac{T-t'}{m-t'}$.
15.	41.25	15.55	17.52	13.04
16.	71.05	15.7	19.84	13.37
17.	102.6	16.2	22.7	13.29

The values contained in the last column of these Tables show that the measurements are, perhaps, not sufficient for the solution of the question raised. In the case of the universal validity of Wiedemann and Franz's law these values must increase considerably with the initial temperatures T ; in that of the validity of Lorenz's law, they must remain constant for all temperatures T .

Now, certainly nothing of an increase of the values is to be

perceived; but, with the irregular differences of the numbers, constancy also is not sufficiently apparent, although it seems more probable. We even see that in this method the errors of observation are greater than are permitted for the decision of so difficult a question. The reason for this is perhaps essentially the employment of an outflow-thermometer. Even the discordant fillings of the tube indicate the uncertainty which is here presented. And then, also, the outflow, notwithstanding the very narrow aperture, did not always follow in the same manner; and so the determination of the principal decisive temperature m could not be effected with the requisite exactness.

§ 4.

All these inconveniences would be avoided with a thermometer of the usual construction, the production of which, however, is more troublesome. I therefore now carried the experiment further with such a thermometer, which I constructed so that a capillary thread which issued at about a third of the length of the tube from the bottom, after a bend, was carried upwards, and then at the height of the upper edge of the tube, after another bend, ran horizontally past a long scale. When the mercury stood in this horizontal part of the thread, there was always present, reckoning the capillary force, a slight excess of pressure towards the upper end of the tube and the thermometer T, which made it impossible for air to penetrate inwards through the fitting-in of the latter. Conversely, in order that the thermometer might not retrograde through the overpressure, and thereby a variable volume of the interior of the tube be obtained, it was held firmly pressed into its fitting by a light stay which could be tightened by means of screws and proceeded from the closure of the tube. The aperture in this latter was of course stopped. At the same time I had shortened the tube, since a calculation of the previous experiments had shown that, in each of them, a very near approximation to the surrounding temperature prevailed in the tube long before reaching the lower end.

The tube, thus converted into a mercury-thermometer with a large vessel, was now, in a bath, carefully calibrated as to its temperature-indications; and it resulted that, on the average, 16 millims. on the scale was equal to one degree. Thus it was made possible to read off the temperatures m rigorously to fractions of a tenth of a degree; and thereby the most decisive part of the apparatus was present in very fine execution. That this large thermometer did not undergo any gradual alteration during the experiments was shown by the circumstance that when, after a day of experiments, the whole thermometer had during

the night resumed the temperature of its environment, it always gave closely correct indications; and, finally, another careful calibration of the tube, after the completion of the experiments, led very rigorously to the previous values.

In like manner I now arranged also the second tube as a large thermometer, and had in its indications of temperature (14 mil-lims. went to 1 degree) the surest determination of the temperatures t' , whether I operated with or without a current.

The apparatus thus altered gave the results contained in the following Tables, which were likewise attained without a current passing. The numbers were gained in several ascending and descending observations, and are here arranged according to the temperatures T.

TABLE IV.

No.	T.	t' .	m .	$\frac{T-t'}{m-t'}$
18.	45.7	16.5	20.27	7.73
19.	49.9	16.76	21.08	7.67
20.	58.8	17.05	22.59	7.54
21.	58.8	17.4	23.1	7.29
22.	58.8	17.08	22.59	7.57
23.	61.2	16.63	22.46	7.65
24.	78.1	17.34	25.4	7.54
25.	78.2	17.6	25.9	7.30
26.	105.1	17.1	28.72	7.58
27.	107.1	17.27	29.29	7.47
28.	127.4	18.09	32.67	7.49
29.	128.4	18	32.6	7.56
30.	129.4	17.62	32.53	7.49
31.	129.4	18.62	33.41	7.49
32.	129.6	17.77	32.66	7.51
33.	130.4	17.48	32.41	7.56
34.	130.7	17.62	32.66	7.55
35.	130.8	18.77	33.63	7.54
36.	131.4	18.8	33.74	7.54
37.	131.4	18.59	33.53	7.55
38.	131.9	18.8	33.84	7.52
39.	132.6	18.2	33.5	7.48
40.	155.7	19.2	37.26	7.55
41.	157.8	19.27	37.69	7.52
42.	158.8	19.33	37.9	7.51
43.	158.8	19.3	37.88	7.51
44.	159.2	19.33	37.97	7.50
45.	159.4	19.36	38.03	7.50
	163.1	18.62	38.48	7.27
	165.2	18.7	38.73	7.31
	168.3	18.55	39.35	7.20
	168.5	18.48	39.35	7.19

The numbers of the last column clearly decide the present question thus, that the heat-conducting power of pure mercury

between 40° and 160° is perfectly *constant*. The small deviations which in the very sensitive lower temperatures are hardly avoidable, lie principally a little more above the mean of all the values 7.525.

The cause of the decided decrease of the values of $\frac{T-t'}{m-t'}$ from 160° upwards (where the horizontal line runs across the Table, and the numbering ceases) is simply that there isolated air-bubbles became visible, developed at so high a temperature from the uppermost mercury of the tube, and having been till then absorbed. These air-bubbles of course cause too high a temperature m to be read off, and thereby lower the values of $\frac{T-t'}{m-t'}$; for boiling the mercury in the tube was not very feasible, since the thermometer T , extending to only 200° , formed a closure to it. On this account I had, before the experiments, heated the mercury of the tube continuously only to 200° , in order to expel the absorbed air. This was therefore satisfactorily accomplished up to temperatures T of 160° ; for the immediate decided fall of the values of $\frac{T-t'}{m-t'}$ as soon as air-bubbles were seen, sufficiently indicates that this circumstance played no important part so long as the values showed no noticeable diminution. Moreover, in the experiments Nos. 42–45, a remote intimation of this behaviour is perhaps given.

Accordingly, in this apparatus the behaviour of mercury cannot be tested beyond 160° .

In order to make more apparent how variable the heat-conducting power of mercury, in opposition to the results we have gained, would be, if it corresponded to the electric conduction, in the following Table the values shall be given on the hypothesis of the variability of both conductivities being equal. As an approximate mean of the various values given (E. Becquerel* 0.00104, Müller† 0.00119, Schröder van der Kolk‡ 0.00086, and Siemens§ 0.00098) for the variability β with mercury, 0.001 shall be taken, and we will put $\alpha = \beta$. With this and with the once for all adopted mean value $t' = 17^{\circ}$, the quotient in the middle column,

$$\frac{\sqrt{3 + 2\alpha T + \alpha t'}}{1 + \alpha T},$$

is calculated, which only in the case of $t' = \theta$ in the above formula contains the variability. The last column then contains,

* Pogg. Ann. vol. lxx. p. 248.

† Ibid. vol. cx. p. 476.

‡ Ibid. vol. lxxiii. p. 440.

§ Ibid. vol. cxiii. p. 104.

starting from the mean value 7.52 put for 160° of the ratio $\frac{T-t'}{m-t'}$, the values of this ratio for the lower temperatures calculated from the formula

$$\text{const} = \frac{T-t'}{m-t'} \cdot \frac{\sqrt{3+2\alpha T+\alpha t'}}{1+\alpha T}.$$

TABLE V.

T.	$\frac{\sqrt{3+2\alpha T+\alpha t'}}{1+\alpha T}$	$\frac{T-t'}{m-t'}$
40	1.6921	7.00
50	1.6814	7.04
80	1.6504	7.18
100	1.6305	7.26
130	1.602	7.39
150	1.5837	7.48
160	1.5748	7.52

The results of experiment thus exclude the possibility of a correspondence between the two conductivities. Besides, for Tables I.-III. twice as great an increase of the values $\frac{T-t'}{m-t'}$ would in this case be found; so that those Tables also pronounce pretty clearly in the same sense.

As the question in the case of mercury was in this most simple manner decided, further experiment with the simultaneous passage of a current would evidently have been superfluous. Preliminary experiments, made with this view, had also presented difficulties arising from the circumstance that, on the passage of the current from the mercury to the iron, and *vice versâ*, strong heating entered, which was not avoided by directing the current in short and equal intervals. Doubtless the surface of the iron was partially oxidized and thereby a considerable resistance opposed to the current. It will be generally difficult to obtain equal raisings of temperature by the current, if such places of passage from one material to another come into play.

This inconvenience drops away completely when the investigation here discussed is applied to a solid metal rod. There no transition-places are present, and the action of the current can be very regularly observed. I am occupied in arranging experiments of this sort. The measurements are exactly analogous to those here communicated. The expansion of the solid metal is used for the determination of the temperatures m and t' , that

expansion being transmitted to a very delicate indicator. I hope hereafter to communicate the results of such experiments as may show how far the other metals differ in their behaviour from mercury.

LXVI. *The Mathematical and Philosophical State of the Physical Sciences.* By PROFESSOR JOSEPH LOVERING*.

THE luminiferous æther and the undulatory theory of light have always troubled what is supposed to be the imperturbable character of the mathematics. The proof of a theory is indisputable when it can predict consequences, and call successfully upon the observer to fulfil its prophecies. It is the boast of astronomers that the law of gravitation thus vindicates itself. The undulatory theory of light has shown a wonderful facility of adaptation to each new exigency in optics, and has opened the eye of observation to see what might never have been discovered without the promptings of theory. But this doctrine, and that of gravitation also, have more than once been arrested in their swift march and obliged to show their credentials. After Fresnel and Young had secured a firm foothold for Huyghens's theory of light in mechanics and experiment, questions arose which have perplexed, if not baffled, the best mathematicial skill. How is the æther affected by the gross matter which it invests and permeates? Does it move when they move? If not, does the relative motion between the æther and other matter change the length of the undulation or the time of oscillation? These queries cannot be satisfactorily answered by analogy; for analogy is in some respects wanting between the æther and any other substance. Astronomy says that aberration cannot be explained unless the æther is at rest. Optics replies that refraction cannot be explained unless the æther moves. Fresnel produced a reconciliation by a compromise:—the æther moves with a *fractional* velocity large enough to satisfy refraction, but too small to disturb sensibly the astronomer's aberration. In 1814, Arago reported to Fresnel that he found no sensible difference in the prismatic refraction of light, whether the earth was moving with full speed toward a star or in the opposite direction, and asked for an explanation. Fresnel submitted the question to mathematical analysis, and demonstrated that, whatever change was produced by the motion of the prism in the relative velocity of

* From the Presidential Address of Professor Lovering before the American Association at Hartford, August, 1874 (*Silliman's American Journal*, October, 1874, p. 297).

light, in the wave-length through the prism, and in the refraction, was compensated by the physiological aberration when the rays emerged. Very recently, Ketteler of Bonn has gone over the whole ground again with great care, studying not only Arago's case but the general one, in which the direction of the light made any angle with the motion of the earth; and he proves that the light will always enter the eye in the same apparent direction as it would have done if the earth were at rest. The mathematical and physical view taken of this subject by Fresnel has been under discussion for sixty years; and forty eminent physicists and mathematicians might be enumerated who have taken part in it. Fresnel's explanation has encountered difficulties and objections. Still it is consistent not only with Arago's negative result, but with the experiments on diffraction by Fizeau and Babinet, and the preponderance of mathematical evidence is on that side. Mr. Huggins runs counter to the general drift of physical and algebraical testimony (although he appears to be sustained by the high authority of Maxwell), when he attributes some displacement of the spectrum-lines to the motion of the earth, and qualifies the observed displacement on that account. The number of stars which Huggins has observed is insufficient for any sweeping generalization. And yet he seems inclined to explain the revelations of his spectroscope, not by the motion of the stars, but by that of the solar system, because those stars which are in the neighbourhood of the place in which astronomers have put the solar apex are moving, apparently, toward the earth, while those in the opposite part of the sky recede. If it be true that the earth's annual motion produces no displacement in the spectrum, then the motion of the solar system produces none. Or, waiving this objection, if the correct explanation has been given by Huggins, astronomers have failed, by their geometrical method, of rising to the full magnitude of the sun's motion. The discrepancy appears to awaken no distrust in Mr. Huggins's mind as to the delicacy of the spectrum-analysis or the mathematical basis of his reasoning. On the contrary, he would remove the discrepancy by throwing discredit on the estimates of star-distances made independently by Struve and Argelander from different lines of thought.

Next we ask if it is certain that even the motion of the luminary will change the true wave-length, the period of oscillation, and the refrangibility of the light which issues from it? The commonly received opinion on this subject has not been allowed to pass unchallenged. It is fortified by more than one analogy; but it is said that comparison is not always a reason. It is not denied that, when the sonorous body is approaching, the sound-waves are shortened, the number of impulses on the ear by the

condensed air is increased, and the pitch of the sound is raised. Possibly the colour of light would follow the same law; but there is no experiment to prove it, and very little analogy exists between the eye and the ear. There is no analogy whatever between the subjective sensation by either organ and the physical action of the prism. The questions at issue are these:—Does refraction depend upon the absolute or the relative velocity of light? are the time of oscillation of the particles of æther and the normal wave-length, corresponding to it, changed by any motion of translation in the origin? or is the conservation of these elements an essential attribute of the luminiferous medium? It has been said that Doppler reasoned as if the corpuscular theory of light were true, and then expressed himself in the language of undulations. Evidently there is an obscurity in the minds of many physicists, and an uncertainty in all, when they reason upon the mechanical constitution of the æther, and the fundamental laws of light. The mathematical theory is not so clear as to be able to dispense with the illumination of experiment. Within the present year, Van der Willigen has published a long and well-considered memoir on the theoretical fallacies which vitiate the whole of Huggins's argument for the motion of the stars and nebulae. His analysis proves that the motion of the luminary will not interfere with the time of oscillation and the wave-length, provided that the origin of the disturbance is not a mathematical point but a vibrating molecule, and that the sphere of action of this molecule upon surrounding molecules is large enough to keep them under its influence during ten or a hundred vibrations, before it is withdrawn by the motion of translation. If this theoretical exposition of the subject should be generally adopted by mathematicians, the spectroscopic observations on the supposed motion of the stars must receive another interpretation. On the other hand, if a luminary is selected which is known to move, independently of spectroscopic observations, and the displacement of the spectrum-lines accords with this motion, it will be time to reconsider the mathematical theory, and make our conceptions of the æther conform to the experiment. The spectroscopic observation of Ångström on an oblique electric spark does not favour Huggins's views. Secchi testifies to opposite displacements when he examined, with a direct-vision spectroscope, the two edges of the sun's equator, one of which was rotating toward him and the other from him; and Vogel has repeated the observation with a reversion-spectroscope. This would have the force of a crucial experiment were it not that an equal displacement was seen on other parallels of latitude, and that the bright bands of the chromosphere were moved, but not the dark lines of the solar atmosphere.

When Voltaire visited England in 1727 he saw at the universities the effect of Newton's revolutionary ideas in astronomy. The mechanism of gravitation had exiled the fanciful vortices of Descartes, which were still circulating on the continent. So he wrote :—" A Frenchman who comes to London finds many changes in philosophy as in other things : he left the world full ; he finds it empty." The same comparison might be made now, not so much between nationalities as between successive stages of scientific development. At the beginning of this century the universe was as empty as an exhausted receiver ; now it has filled up again. Nature's abhorrence of a vacuum has been resuscitated, though for other reasons than those which satisfied the Aristotelians. It is the mathematicians, and not the metaphysicians, who are now discussing the relative merits of the *plenum* and the *vacuum*. Newton, in his third letter to Bentley, wrote in this wise :—" That gravity should be innate, inherent and essential to matter, so that one body may act upon another at a distance, through a *vacuum*, without the mediation of any thing else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man, who has in philosophical matters a competent faculty of thinking, can ever fall into it." Roger Cotes, who was Newton's successor in the chair of Mathematics and Natural Philosophy at Cambridge, was only four years old when the first edition of the *Principia* was issued ; and Newton outlived him by ten years. The venerable teacher pronounced upon the young mathematician, his pupil, these few but comprehensive words of eulogy :—" If Cotes had lived, we should have known something." The view taken of gravitation by Cotes was not the same as that held by his master. He advocated the proposition that action at a distance must be accepted as one of the primary qualities of matter, admitting of no further analysis. It was objected by Hobbes and other metaphysicians, that it was inconceivable that a body should act where it was not. All our knowledge of mechanical forces is derived from the conscious effort we ourselves make in producing motion. As this motion employs the machinery of contact, the force of gravitation is wholly outside of all our experience. The advocates of action at a distance reply that there is no real contact in any case, that the difficulty is the same with the distance of molecules as that of planets, that the mathematics are neither long-sighted nor short-sighted, and that an explanation which suits other forces is good enough for gravitation.

Comte extricated himself from this embarrassment by excluding causes altogether from his positive philosophy. He rejects the word attraction as implying a false analogy, inconsistent with

Newton's law of distance. He substitutes the word gravitation, but only as a blind expression by which the facts are generalized. According to Comte's philosophy, the laws of Newton are on an equality with the laws of Kepler, only they are more comprehensive, and the glory of Kepler has the same stamp as that of Newton. Hegel, the eminent German metaphysician, must have looked at the subject in the same light when he wrote these words :—"Kepler discovered the laws of free motion ; a discovery of immortal glory. It has since been the fashion to say that Newton first found out the truth of these rules. It has seldom happened that the honour of the first discoverer has been more unjustly transferred to another." Shelling goes further in the same direction : he degrades the Newtonian law of attraction into an empirical fact, and exalts the laws of Kepler into necessary results of our ideas.

Meanwhile the Newtonian theory of attraction, under the skilful generalship of the geometers, went forth on its triumphal march through space, conquering great and small, far and near, until its empire became as universal as its name. The whirlpools of Descartes offered but a feeble resistance, and were finally dashed to pieces by the artillery of the parabolic comets ; and the rubbish of this fanciful mechanism was cleaned out as completely as the cumbrous epicycles of Ptolemy had been dismantled by Copernicus and Kepler. The mathematicians certified that the solar system was protected against the inroads of comets, and the border warfare of one planet upon another, and that its stability was secure in the hands of gravitation, if only space should be kept open, and the dust and cobwebs which Newton had swept from the skies should not reappear. Prophetic eyes contemplated the possibility of an untimely end to the revolution of planets, if their ever-expanding atmospheres should rush in to fill the room vacated by the maelstroms of Descartes. When it was stated that the absence of infinite divisibility in matter, or the coldness of space would place a limit upon expansion, and, at the worst, that the medium would be too attenuated to produce a sensible check in the headway of planets, and when in more recent times even Encke's comet showed but the slightest symptoms of mechanical decay, it was believed that the motion was, in a practical, if not in a mathematical sense, perpetual. Thus it was that the splendours of analysis dimmed the eyes of science to the intrinsic difficulties of Newton's theory, and familiarity with the language of attraction concealed the mystery that was lurking beneath it. A long experience in the treatment of gravitation has supplied mathematicians with a fund of methods and formulas suited to similar cases. As soon as electricity, magnetism, and electromagnetism

took form, they also were fitted out with a garment of attractive and repulsive forces acting at a distance; and the theories of Cavendish, Poisson, Æpinus and Ampère, indorsed as they were by such names as Laplace, Plana, Liouville, and Green, met with general acceptance.

The seeds, which were destined to take root in a later generation, and disturb, if not dislodge, the prevalent interpretation of the force of gravitation, were sown by a contemporary of Newton. They found no congenial soil in which they could germinate and fructify until the early part of this century. At the present moment we find the luminiferous æther in quiet and undivided possession of the field from which the grosser material of ancient systems had been banished. The *plenum* reigns everywhere; the vacuum is nowhere. Even the corpuscular theory of light, as it came from the hands of its founder, required the reinforcement of an æther. Electricity and magnetism, on a smaller scale, applied similar machinery. If there was a fundamental objection to the conception of forces acting at a distance, certainly the bridge was already built by which the difficulty could be surmounted. The turning-point between the old physics and the new physics was reached in 1837, when Faraday published his experiments on the specific inductive capacity of substances. This discovery was revolutionary in its character; but it made no great stir in science at the time. The world did not awake to its full significance until the perplexing problem of ocean-telegraphs converted it from a theoretical proposition into a practical reality, and forced it on the attention of electricians. The eminent scientific advisers of the cable-companies were the first to do justice to Faraday. This is one of the many returns made to theoretical electricity for the support it gave to the most magnificent commercial enterprise.

The discovery of diamagnetism furnished another argument in favour of the new interpretation of physical action. What that new interpretation was is well described by Maxwell. "Faraday, in his mind's eye, saw lines of force traversing all space, where the mathematicians saw centres of force attracting at a distance; Faraday saw a medium where they saw nothing but distance; Faraday sought the seat of the phenomena in real actions going on in the medium; they were satisfied that they had found it in a *power* of action at a distance impressed on the electric fluids." The physical statement waited only for the coming of the mathematicians who could translate it into the language of analysis and prove that it had as precise a numerical consistency as the old view with all the facts of observation. A paper published by Sir William Thomson, when he was an Undergraduate at the University of Cambridge, pointed the way. Prof. Maxwell, in

his masterly work on electricity and magnetism, which appeared in 1873, has built a monument to Faraday, and unconsciously to himself also, out of the strongest mathematics. For forty years mathematicians and physicists had laboured to associate the laws of electrostatics and electrodynamics under some more general expression. An early attempt was made by Gauss in 1835; but his process was published, for the first time, in the recent complete edition of his works. Maxwell objects to the formula of Gauss because it violates the law of the conservation of energy. Weber's method was made known in 1846; but it has not escaped the criticism of Helmholtz. It represents faithfully the laws of Ampère and the facts of induction, and led Weber to an absolute measurement of the electrostatic and electromagnetic units. The ratio of these units, according to the formulas, is a velocity; and experiment shows that this velocity is equal to the velocity of light. As Weber's theory starts with the conception of action at a distance, without any mediation, the effect would be instantaneous, and we are at a loss to discover the physical meaning which he attaches to his velocity. Gauss abandoned his researches in electromagnetism because he could not satisfy his mind in regard to the propagation of its influence in time. Other mathematicians have worked for a solution, but have lost themselves in a cloud of mathematical abstraction. The two theories of light have exhausted all imaginable ways in which force can be gradually transmitted without increase or loss of energy. Maxwell cut the Gordian knot when he selected the luminiferous æther itself as the arena on which to marshal the electromagnetic forces under the symbols of his mathematics, and made light a variety of electromagnetic action. His analysis gave a velocity essentially the same as that of Weber, with the advantage of being a physical reality and not a mere ratio. Of the two volumes of Mr. Maxwell, freighted with the richest and heaviest cargo, the reviewer says:—"Their author has, as it were, flown at every thing; and, with immense spread of wing and power of beak, he has hunted down his victims in all quarters, and from each has extracted something new and interesting for the intellectual nourishment of his readers." Clear physical views must precede the application of mathematics to any subject. Maxwell and Thomson are liberal in their acknowledgments to Faraday. Mr. Thomson says:—"Faraday, without mathematics, divined the result of the mathematical investigation; and, what has proved of infinite value to the mathematicians themselves, he has given them an articulate language in which to express their results. Indeed the whole language of the *magnetic field and lines of force* is Faraday's. It must be said for the mathematicians that they greedily accepted it, and

have ever since been most zealous in using it to the best advantage."

It is not expected that the new views of physics will be generally accepted without vigorous opposition. A large amount of intellectual capital has been honestly invested in the fortunes of the other side. The change is recommended by powerful physical arguments, and it disenthralles the theories of science from many metaphysical difficulties which weigh heavily on some minds. On the other hand, the style of mathematics which the innovation introduces is novel and complex; and good mathematicians may find it necessary to go to school again before they can read and understand the strange analysis. It is feared that with many, who are not easily deflected from the old ruts, the intricacies of the new mathematics will outweigh the superiority of the new physics.

The old question, in regard to the nature of gravitation, was never settled; it was simply dropped. Now it is revived with as much earnestness as ever, and with more intelligence. Astronomy cast in its own mould the original theories of electrical and magnetic action. The revolution in electricity and magnetism must necessarily react upon astronomy. It was proved by Laplace, from data which would now probably require a numerical correction, that the velocity of the force of gravitation could not be less than eight million times the velocity of light; in fact, that it was infinite. Those who believe in action at a distance cannot properly speak of the transmission of gravitation. Force can be transmitted only by matter, either with it or through it. According to their view, action at a distance is the force, and it admits of no other illustration, explanation, or analysis. It is not surprising that Faraday and others, who had lost their faith in action at short distances, should have been completely staggered by the ordinary interpretation of the law of gravitation, and that they declared the clause which asserted that the force diminished with the square of the distance to be a violation of the principle of the conservation of energy.

Must we, then, content ourselves with the naked facts of gravitation, as Comte did? or is it possible to resolve them into a mode of action in harmony with our general experience, and which does not shock our conceptions of matter and force? In 1798, Count Rumford wrote thus:—"Nobody surely, in his sober senses, has ever pretended to understand the mechanism of gravitation." Probably Rumford had never seen the paper of Le Sage, published by the Berlin Academy in 1782, in which he expounded his mechanical theory of gravitation, to which he had devoted sixty-three years of his life. In a posthumous work, printed in 1818, Le Sage has developed his views more fully.

He supposed that bodies were pressed toward one another by the everlasting pelting of ultramundane atoms, inward bound from the immensity of space beyond, the faces of the bodies which looked towards each other being mutually screened from this bombardment. It was objected to this hypothesis, which introduced Lucretius into the society of Newton and his followers, that the collision of atoms with atoms and with planets, would cause a secular diminution in the force of gravity. Le Sage admitted the fact. But as no one knew that the solar system was eternal, the objection was not fatal. As the necessity for giving a mechanical account of gravitation was not generally felt at the time, the theory of Le Sage fell into oblivion. In 1873 Sir William Thomson resuscitated and republished it. He has fitted it out in a fashionable dress, made out of elastic molecules instead of hard atoms, and has satisfied himself that it is consistent with modern thermodynamics and a perennial gravitation.

Let us now look in a wholly different quarter for the mechanical origin of gravitation. In 1870 Prof. Guthrie gave an account of a novel experiment, viz. the attraction of a light body by a tuning-fork when it was set in vibration. Thomson repeated the experiment upon a suspended eggshell and attracted it by a simple wave of the hand. Thomson remarks "that what gave the great charm to these investigations, for Mr. Guthrie himself, and no doubt also for many of those who heard his expositions and saw his experiments, was, that the results belong to a class of phenomena to which we may hopefully look for discovering the mechanism of magnetic force, and possibly also the mechanism by which the forces of electricity and gravity are transmitted." By a delicate mathematical analysis, Thomson arrives at the theorem that the "average pressure at any point of an incompressible, frictionless fluid originally at rest, but set in motion and kept in motion by solids, moving to and fro, or whirling round in any manner, through a finite space of it," would explain the attractions just described. Moreover he is persuaded by other effects besides those of light, that, in the interplanetary spaces and in the best artificial vacuum, the medium which remains has "perfectly decided mechanical qualities, and, among others, that of being able to transmit mechanical energy in enormous quantities;" and he cherishes the hope that his mathematical theorems on abstract hydrokinetics are of some interest in physics as illustrating the great question of the eighteenth and nineteenth centuries—Is action at a distance a reality, or is gravitation to be explained, as we now believe magnetic and electric forces must be, by action of intervening matter?

In 1869 and 1873, Prof. Challis of Cambridge, England, published two works on the Principles of Mathematical Physics. They embody the mature reflections of a mathematical physicist at the advanced age of three score years and ten. Challis believes that there is sufficient evidence for the existence of æther and atoms as physical realities. He then proceeds to say:—"The fundamental and only admissible idea of *force* is that of *pressure*, exerted either actively by the æther against the surface of the atoms, or as reaction of the atoms on the æther by resistance to that pressure. The principle of deriving fundamental physical conceptions from the indications of the senses does not admit of regarding *gravity*, or any other force varying with distance, as an essential quality of matter, because, according to that principle, we must, in seeking for the simplest idea of physical force, have regard to the sense of *touch*. Now, by this sense we obtain a perception of force as pressure, distinct and unique, and not involving the variable element of distance, which enters into the perception of force as derived from the sense of sight alone. Thus, on the ground of simplicity as well as of distinct perceptibility, the fundamental idea of force is pressure." As all other matter is passive except when acted upon by the æther, the æther itself, in its quiescent state, must have uniform density. It must be coextensive with the vast regions in which material force is displayed. Challis had prepared himself for the elucidation and defence of his dynamical theory by a profound study of the laws of motion in elastic fluids. From the mathematical forms in which he has expressed these laws he has attempted to derive the principal experimental results in light, heat, gravitation, electricity, and magnetism. Some may think that Mr. Challis has done nothing but clothe his theory in the cast-off garments of an obsolete philosophy. If its dress is old, it walks upon new legs. The interplay between æther and atoms is now brought on to the stage, not as a speculation sustained by metaphysical and theological arguments, but as a physical reality with mathematical supports. I should do great injustice to this author if I left the impression that he himself claimed to have covered the whole ground of his system by proof. Mathematical difficulties prevented him from reaching a numerical value for the resultant action of a wave of æther upon the atom. What he has written is the guide-post, pointing the direction in which science is next to travel; but the end of the journey is yet a great way off. The repeated protests of Mr. Challis against the popular physics of the day, and his bold proclamation of the native, independent motion of the æther, have aroused criticism. What prevents the free æther, asks the late Sir John Herschel, from expanding into infinite space? Mr. Challis replies that we know nothing about

infinite space or what happens there; but the existence of the æther, where our experience can follow it, is a physical reality. The source of the motion which the æther acquires is not the sun; for the most efficient cause of solar radiation is gravitation and condensation. Our author avoids the vicious circle of making gravitation first the reason, and afterwards the consequence of the motion of the æther. He says:—"It follows that the sun's heat, and the heat of masses in general, are stable quantities, oscillating, it may be, like the planetary motions, about *mean* values, but never permanently changing, so long as the Upholder of the universe conserves the force of the æther and the qualities of the atoms. There is no law of destructibility; but the same Will that conserves can in a moment destroy." The following remarks upon this theory deserve our attention:—"The explanation of any action between distant bodies by means of a clearly conceivable process, going on in the intervening medium, is an achievement of the highest scientific value. Of all such actions that of gravitation is the most universal and the most mysterious. Whatever theory of the constitution of bodies holds out a prospect of the ultimate explanation of the process by which gravitation is effected, men of science will be found ready to devote the whole remainder of their lives to the development of that theory."

The hypotheses of Challis and Le Sage have one thing in common; the motion of the æther and the driving storm of atoms must come from outside the world of stars. "On either theory the universe is not even temporarily automatic, but must be fed from moment to moment by an agency external to itself." Our science is not a finality. The material order which we are said to know makes heavy drafts upon an older or remoter one, and that again upon a third. The world, as science looks at it, is not self-sustaining. We may abandon the hope of explaining gravitation, and make attraction itself the primordial cause. Our refuge then is in the sun. When we qualify the conservation of energy by the dissipation of energy, the last of which is as much an induction of science as the first, the material fabric which we have constructed still demands outward support. Thomson calculates that, within the historical period, the sun has emitted hundreds of times as much mechanical energy as is contained in the united motions of all the planets. This energy, he says, is dissipated more and more widely through endless space, and never has been, probably never can be, restored to the sun without acts as much beyond the scope of human intelligence as a creation or annihilation of energy, or of matter itself, would be.

From the earliest dawn of intellectual life a general theory of

the constitution of matter has been a fruitful subject of debate, and human science and philosophy have ever been dashing their heads against the intractable atoms. The eagerness of the discussion was the greater the more hopeless the solution. For every man who set up an hypothesis upon the subject there were half a dozen others to knock it down, until at last speculation, which bore no fruit, was suspended. A lingering interest still hung around the question whether matter was not infinitely divisible, and the atomic philosophers were not chasing a chimaera. From every new decision on this single point there was an appeal; and the foothold which the atoms had secured in chemistry was gradually subsiding. Of a sudden the atomic theory has gained a new lease of life. But the hero of the new drama is not the atom, but the molecule. In all the physical sciences, including astronomy, the war has been carried home to the molecules, and the intellectual victories of this and the next generation will be on this narrow field. From the outlying provinces of physics, from the sun, the stars, and the nebulae, from the comets and meteors, from the zodiacal light and the aurora, from the exquisitely tempered and mysterious æther the forces of nature have been moving in converging lines to this common battle-ground; and some shouts of victory have already been heard. In the long and memorable controversy between Newton and Leibnitz, and their adherents, as to the true measure of force, it was charged against the Newtonian rule that force was irrecoverably lost whenever a collision occurred between hard, inelastic bodies. The answer was, that nature had anticipated the objection and had avoided this kind of matter. Inelastic bodies were yielding bodies; and the force which had disappeared from the motion had done its work in changing the shape. But unless the body could recover its original figure by elasticity, there was no potential energy and force was annihilated. It is now believed, and to a large extent demonstrated, that the force, apparently lost, has been transformed into heat, electricity, or some other kind of molecular motion, of which the change of shape is only the outward sign. The establishment on a firm foundation of theory and experiment of the so-called conservation of energy, the child of the correlation of physical forces, is one of the firstfruits of molecular mechanics.

It is no disparagement of this discovery, on which was concentrated the power of several minds, to call it an extension, though a vast one, of Newton's law of inertia, of Leibnitz's *vis viva*, and of Huyghens's and Bernoulli's conservation of living forces—these older axioms of mechanics having free range only in astronomy, where friction, resistance, and collision do not

interfere. The conservation of energy, in its extended signification, promises to be, like its forerunners, a valuable guide to discovery, especially in the dark places into which physical science has now penetrated. The caution which Lagrange has given in reference to similar mechanical principles, such as the conservation of the motion of the centre of gravity, the conservation of moments of rotation, the preservation of areas, and the principle of least action, is not without its applicability to the new generalization. Lagrange accepts them all as results of the known laws of mechanics and not as the essence of the laws of nature. The most that physical science can assert is that it possesses no evidence of the destructibility of matter or force.

It is not pretended that the existence of atoms has been or can be proved or disproved. Some chemists think that the atomic theory is the life of chemistry; others have abandoned it. Its importance is lost in that of the molecular theory. And what has this accomplished to justify its existence? If we define the molecule of any substance as the smallest mass of that substance which retains all its chemical properties, we can start with the extensive generalization of Avogadro and Ampère, that the same volume of every kind of matter in the state of vapour, and under the same pressure and temperature, contains an equal number of such molecules. The conception of matter as consisting of parts, which are perpetually flying over their microscopic orbits and producing by their fortuitous concurrence all the observed qualities of bodies, is as old as Lucretius. He saw the magnified symbol of his hypothesis in the motes which chase one another in the sunbeam. One of the Bernoullis thought that the pressure of gases might be caused by the incessant impact of these little masses on the vessel which held them. The discovery that heat was a motion and not a substance, foreshadowed by Bacon, made probable by Rumford and Davy, and rigidly proved by Mayer and Joule when they obtained its exact mechanical equivalent, opened the way to the dynamical theory of gases. Joule calculated the velocity of this promiscuous artillery, rendered harmless by the minuteness of the missiles, and found that the boasted guns of modern warfare could not compete with it. Clausius consummated the kinetic theory of gases by his powerful mathematics, and derived from it the experimental laws of Mariotte, Gay-Lussac, and Chasles. By the assumption of data more or less plausible, several mathematicians have succeeded in computing the sizes and the masses of the molecules and some of the elements of their motion. It should not be forgotten that mathematical analysis is only a rigid system of logic, by which wrong premises conduct the more surely

to an incorrect conclusion. To claim for all the conclusions which have been published in relation to the molecules the certainty which fairly belongs to some of them would prejudice the whole cause.

One of the most interesting investigations in molecular mechanics was published by Helmholtz in 1858. It is a mathematical discussion of what he calls ring vortices in a perfect, frictionless fluid. Helmholtz has demonstrated that such vortices possess a perpetuity and an inviolability once thought to be realized only by the eternal atoms. The ring-vortices may hustle one another, and pass through endless transformations, but they cannot be broken or stopped. Thomson seized upon them as the impersonation of the indestructible but plastic molecule which he was looking for to satisfy the present condition of physical science. The element of the new physics is not an atom or a congeries of atoms, but a whirling vapour. The molecules of the same substance have one invariable and unchangeable mass; they are all tuned to one standard pitch, and, when incandescent, emit the same kind of light. The music of the spheres has left the heavens and condescended to the rhythmic molecules. There is here no birth or death or variation of species. If other masses than the precise ones which represent the elements have been eliminated, whither, asks Maxwell, have they gone? the spectroscope does not show them in the stars or nebulae; the hydrogen and sodium of remotest space are in unison with the hydrogen and sodium of earth.

In the phraseology of our mechanics we define matter and force as if they had an independent existence. But we have no conception of inert matter or of disembodied force. All we know of matter is its pressure and its motion. The old atom had only potential energy; the energy of its substitute, the molecule, is partly potential and partly kinetic. If it could be shown that all the phenomena displayed in the physical world were simply transmutations of the original energy existing in the molecules, physical science would be satisfied. Where physical science ends, natural philosophy, which is not wholly exploded from our vocabulary, begins. Natural philosophy can give no account of energy when disconnected with an ever present Intelligence and Will. In Herschel's beautiful dialogue on atoms, after one of the speakers had explained all the wonderful exhibition of nature as the work of natural forces, Hermione replies, "Wonderful, indeed! Anyhow, they must have not only good memories but astonishing presence of mind, to be always ready to act, and always to act, without mistake, according to the primary laws of their being, in every complication that occurs." And elsewhere, "Action, without will or effort,

is to us, constituted as we are, unrealizable, unknowable, inconceivable." The monads of Leibnitz and the demons of Maxwell express in words the personality implied in every manifestation of force.

LXVII. *On Temperament, or the Division of the Octave.*

By MR. R. H. M. BOSANQUET*.

THE subjects of the paper were some points in the theory of the division of the octave, and the employment of a certain notation.

The following definitions are fundamental:—

"Regular Systems" are such that all their notes can be arranged in a continuous series of fifths.

"Regular Cyclical Systems" are not only regular, but return into the same pitch after a certain number of fifths. Every such system divides the octave into a certain number of equal intervals.

"Error" is deviation from a perfect interval.

"Departure" is deviation from an equal-temperament interval.

E. T. is used as an abbreviation for "equal temperament."

Departure is called "positive" when intervals are sharper than E. T., "negative" when flatter than E. T.

Systems are called "positive" when the departure of twelve fifths is positive, "negative" when the departure of twelve fifths is negative.

Systems are said to be "of the r th order," positive or negative, when the departure of twelve fifths is $\pm r$ units of the system.

Negative systems form their major thirds in accordance with the ordinary notation of music; for the departure of major thirds is negative, *i. e.* they are flatter than E. T. thirds. And if we take four negative fifths up, we have a third with negative departure; thus, C \sharp is either the third to A or 4 fifths up from A.

Positive systems form their thirds by 8 fifths down; for 8 positive fifths down give a negative departure. Thus the third of A should be D \flat . Hence positive systems require a separate notation. Helmholtz proposes a notation for this purpose, which, however, is unsuitable for use with musical symbols. The following notation is adopted for positive systems. (Perfect and approximately perfect fifths are positive.) The fifths are arranged in series, each con-

* Being an abstract of a paper read before the Musical Association, November 2, 1874. Communicated by the Author.

taining 12 fifths, from $f\sharp$ up to b . The series $f\sharp-b$ is called the unmarked series; the next series of 12 fifths up is affected with the mark (\diagup), the next series of 12 fifths up with ($\diagup\diagup$), and so on; the first series down below the unmarked series with the mark (\diagdown), the next series down with ($\diagdown\diagdown$), and so on. Thus $\diagdown e$ is 8 fifths down from c , and $c-\diagdown e$ is approximately a perfect third. The error of the third $c-\diagdown e$ is about $\frac{1}{50}$ of an E. T. semitone when the fifths are perfect.

This notation is suitable for employment with written music. The following passage is an example:—



The interval $g-\diagdown f$ is a close approximation to the harmonic seventh; $\diagup a\flat-f\sharp$ is rendered very smooth by the employment of the same interval.

A harmonium has been constructed on which such passages can be played, the form of the fingering being the same in all keys.

The notation is also useful for the discussion of some systems of historical interest. Thus we have a scale of F in Mersenne (1636) with eighteen notes to the octave. This possessed the following resources:—

- Major chords of $c-f-b\flat-e\flat$,
 „ $\diagdown e-\diagdown a-\diagdown d-\diagdown g$, thirds to the above,
 „ $\diagup a\flat-\diagup d\flat-\diagup g\flat$, thirds below $c-f-b\flat$.
- Minor chords of $c-f-b\flat-e\flat$,
 „ $\diagdown e-\diagdown a-\diagdown d-\diagdown g$, thirds to the above,
 „ $\diagdown c\sharp-\diagdown f\sharp-\diagdown b$, thirds to $\diagdown a-\diagdown d-\diagdown g$.

We have here the two forms of second of the key, g and $\diagdown g$, differing approximately by a comma. This double form appears in all good attempts at systems with perfect fifths.

Principle of “symmetrical arrangement.”—If we place the E. T. notes in the order of the scale, and set off the departures of the notes of any system at right angles to the E. T. line, sharp departures up and flat departures down, we obtain the positions of a symmetrical arrangement. (A diagram was exhibited with a symmetrical arrangement of the notes of General Thompson’s enharmonic organ.) This symmetrical arrangement is the principle of the keyboard of the harmonium above referred to. The following series of intervals lie on characteristically placed straight lines in a symmetrical arrangement.

Name of interval in general case.	Name when fifths are perfect.	Series of intervals.
2-fifths tone	Major tone	$c-d-e \dots$
5-fifths semitone. . . .	Pythagorean semitone.	$\diagup c-\diagdown c\sharp-d-\diagdown e \dots$
7-fifths semitone. . . .	Apotomè Pythagorica. .	$\diagdown c-c\sharp-d-\diagup e \dots$
Third by 4 fifths up.	Dissonant, or Pythagorean third	$c-e-\diagup a\flat-\diagdown c \dots$
Third by 8 fifths down.	(Approximately perfect third.)	$c-\diagdown e-\diagdown a\flat-\diagdown \diagdown c \dots$

The last example shows us that three perfect thirds fall short of the octave by two commas approximately. (The departure of 12 perfect fifths is a Pythagorean comma = .23460 of an E.T. semitone. The real comma is .21506. These are regarded as approximately equal for present purposes.)

Regular Systems.

The importance of regular systems arises from the symmetry of the scales which they form.

Theorem 1. In any regular system five 7-fifths semitones + seven 5-fifths semitones make an exact octave; for the departures (from E. T.) of the first set are due to 35 fifths up, and those of the second set to 35 fifths down, leaving twelve E. T. semitones, which form an exact octave.

Theorem 2. In any regular system the difference between the 7-fifths semitone and the 5-fifths semitone = departure of 12 fifths, having regard to sign; or

7-fifths semitone – 5-fifths semitone = departure of 12 fifths; for the 7-fifths semitone up from c is got by the fifths,

$$c-g-d-a-b-\diagup f\sharp-\diagup c\sharp;$$

and the 5-fifths semitone down from $\diagup c\sharp$ is got by the fifths,

$$\diagdown c\sharp-\diagdown g\sharp-\diagdown d\sharp-\diagdown a\sharp-\diagdown f-\diagdown c;$$

whence the difference is the interval $c-\diagdown c$; and this is the departure of 12 fifths by definition.

N.B. Here we extend our notation to fifths of any kind, negative as well as positive.

Regular Cyclical Systems.

The importance of regular cyclical systems arises from the infinite freedom of modulation in every direction, which is possible in such systems when properly arranged; whereas in non-cyclical systems required modulations are liable to be impossible, owing to the demand arising for notes outside the material provided.

Theorem 3. In regular cyclical systems of the r th order the difference between the 7-fifths semitone and the 5-fifths semitone is r units of the system, having regard to sign.

For, recalling the definition of r th order (departure of 12 fifths $= \pm r$ units of system), the proposition follows at once from Theorem 2.

This proposition, taken with theorem 1, enables us to ascertain the number of divisions in the octave in systems of any order. The systems mentioned below are all of some historical or other interest; those of higher orders possess but little interest.

Primary (1st order) positive.

7-fifths semitone = s units.	5-fifths semitone = f units.	Number of units in octave (Theorem 1), $5s + 7f$.
2	1	17
3	2	29
4	3	41
5	4	53
6	5	65

Secondary (2nd order), positive.

11	9	118
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Primary Negative.

1	2	19
2	3	31

Secondary Negative.

3	5	50
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The formation of scales and the discussion of the properties of these systems are reserved for a future occasion.

Theorem 4. If a system divide the octave into n equal intervals, the condition that it may be of the r th order is that $r + 7n$ is a multiple of 12; and the number $\frac{r + 7n}{12}$ is the number of units in the fifth of the system.

Let m be the number of units in the fifth.

$\frac{12}{n}$ is the unit in semitones (E. T.);

$\therefore m \cdot \frac{12}{n}$ is the fifth in semitones,

and $12\frac{m}{n} - 7$ the departure of the fifth in semitones.

$$\begin{aligned}
 12\left(12\frac{m}{n} - 7\right) &= \text{departure of 12 fifths in semitones} \\
 &= r \text{ units of system (Def.)} \\
 &= r \cdot \frac{12}{n};
 \end{aligned}$$

whence

$$m = \frac{r+7n}{12};$$

and m is an integer by hypothesis. Whence the proposition.

This formula is useful for finding corresponding values of r and n . The results are the same as those of Theorem 3.

Theorem 5. If a system divide the octave into n equal intervals, the total departure of all the n fifths of the system $= \pm r$ E. T. semitones, where $\pm r$ is the order of the system.

For the departure of 12 fifths $= \pm r$ units

$$= \pm r \frac{12}{n} \text{ semitones; } \dots (\alpha)$$

\therefore departure of n fifths $= \pm r$ semitones.

Cor. Dividing equation (α) by 12, we have

$$\text{Departure of 1 fifth} = \pm \frac{r}{n},$$

a proposition which will be useful hereafter.

This theorem gives rise to a curious method of deriving the various systems.

Suppose the notes of an equal-temperament series arranged in order of fifths and proceeding onwards indefinitely, thus, $c, g, d, a, e, b, f\sharp, c\sharp, g\sharp, d\sharp, a\sharp, f, c, g, \dots$, and so on.

Let a system of fifths, say positive, start from c ; then at each step the system rises further from the E. T. It can only return to c by sharpening an E. T. note.

Suppose that b is sharpened one E. T. semitone so as to become c ; then the return may be effected in 5 fifths, or at the next b in 17 fifths, or at the next in 29, or in 41, or in 53, and so on. These are the primary positive systems. Secondary positive systems may be got by sharpening $b\flat$ by two semitones, and so on. So with negative fifths: to return to c , $c\sharp$ may be depressed a semitone; these returns give 7, 19, 31, \dots fifths; or d may be depressed two semitones, and so on. There remain to be considered in connexion with this subject:—

Points of historical interest.

Formation of scales and properties of systems.

Instrumental means of control.

LXVIII. *On Comets and their Tails.**To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

THE observations of the comets' orbits have, I suppose, abundantly proved that the apparent nucleus is the centre of gravity, and therefore that there is an equal quantity of matter on all sides of the nucleus, and not, as appears to the eye, that the luminous tail is the only matter which radiates from the centre. A comet, then, is a spherical mass of nebulous matter, of which the radius is the length of the visible tail, or even longer. If the matter is compressible, the force of gravitation will make it more dense towards the middle; the middle will thus have more power of reflecting the sun's light, and be more luminous than the parts at a distance from the middle. This will explain the nucleus and the nebulous appearance around it. It remains for us to inquire what are the circumstances which can be supposed to make the less dense portion of the ball luminous on one side only, and give to it the appearance of a tail on the side furthest from the sun, as in our fig. 1.

Fig. 1.

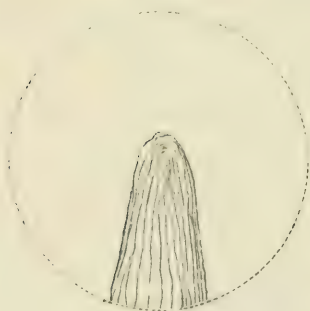
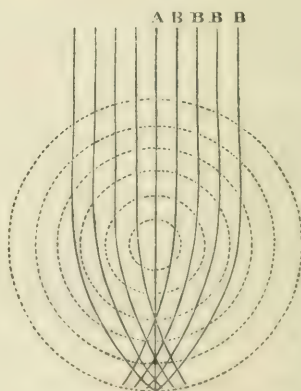


Fig. 2.



Let A in fig. 2 be a ray of light passing from the sun direct towards the centre of a ball of nebulous matter. It will not be deflected on either side, but will mark the central axis of our ball.

Let B be any other ray parallel to the former until it enters the ball. This will be refracted towards the central axis as it passes from each rarer stratum of our ball into one more dense. Let us suppose it to be refracted only so much as to approach the central axis at a place beyond the nucleus. It will then quit

the ball at a place much nearer to the central axis than that at which it entered.

Then, if the reflecting-power of our nebulous ball is so far weak that the rarer portions of matter, which receive the rays singly (as is the case on the side nearest to the sun), are not luminous to our eyes, it is yet quite possible that the portion equally rare on the side furthest from the sun may be luminous to us, because upon that a large number of these rays are concentrated as by a magnifying glass. This part will thus have the appearance of a tail on the side furthest from the sun.

In those few comets which have also a short tail pointing towards the sun, we may suppose that the refracting-power of the matter is so far greater that some of the rays meet in a focus before they reach the centre.

When the tail is curved or otherwise irregular, it may be that the ball is not a perfect sphere.

The above hypothesis, in the hands of some one better able to work it out, might perhaps be made to explain that interesting appearance the comet's tail.

Yours obediently,

32 Highbury Place,
November 19, 1874.

SAMUEL SHARPE.

LXIX. *Researches in Acoustics*.—No. V.

By ALFRED M. MAYER.

[Concluded from p. 452.]

5. *Six Experimental Methods of Sonorous Analysis described and discussed.*

THE remarkable discoveries in sound made in these later times by Helmholtz were owing in great part to his having early seen the necessity of obtaining precise knowledge of the composition of sounds (by means of the methods of sonorous analysis which he had devised) before one could attempt to give an explanation of the causes of timbre, of the mechanism of audition, and of the physiological causes of musical harmony; and, furthermore, he reduced all of the analytic methods and explanations contained in his classical work to a harmonious system, by showing how they naturally flowed from the fertile theorem of Fourier. As Helmholtz distinctly states, "In letzter Instanz ist also der Grund der von Pythagoras aufgefundenen rationellen Verhältnisse in dem Satze von Fourier zu finden, und in gewissem Sinne ist diese Satz als die Urquelle des Generalbasses zu betrachten" (*Tonempfindungen*, p. 346).

The evident importance of the subject of sonorous analysis will probably render interesting the few remarks I here venture to offer on this subject. I will describe in order six methods of sonorous analysis. Methods (1), (2), and (5) have been used by Helmholtz and König. Methods (3), (4), and (6), as far as I know, originated with me. To render comparable all the experiments which I shall describe, I shall always use one composite sound of uniform intensity, by sounding with a blast of constant pressure an Ut_2 Grenié free-reed pipe, from which has been removed its reinforcing pyramidal pipe.

(1) *Analysis by means of Resonators applied directly to the ear.*

This method of Helmholtz is so well known that it need not here be described; but I will give some experiments which I have made on its degree of precision under the head of the sixth method of analysis, to be described.

(2) *Analysis by means of Resonators connected with König's manometric flames.*

This is the least delicate and accurate of all methods of sonorous analysis; but it has a value in giving an objective ocular illustration which is sometimes of use. I do not, however, here refer to the use of a manometric flame placed in connexion with a conical pipe into which one sings, and which instrument, in the hands of König, has done such admirable work in the analysis of the vowel sounds; but I refer to the harmonic series of resonators connected with as many flames which burn near a long revolving mirror.

The following experiments will show the degree of delicacy of the above apparatus. I sounded the reed-pipe with its maximum intensity when it was about one foot from the centre of the series of resonators, and produced well-marked serrations in the Ut_3 and Ut_4 flames; but the other flames showed only slightly indented top borders. It was only when I sang loudly Ut_3 , or sounded this same note on a French horn, that the edges of the flames became deeply serrated.

(3) *Analysis by means of Resonators which are successively brought near the source of origin of a composite sound and thus successively reinforce all of its sonorous elements.*

If we take any two resonators separated by a known interval and vibrate before them the forks of this interval, we can carry these sounds in the memory. Now close the nipples of the resonators with wax and successively bring their mouths near the origin of the composite sound. If the simple sounds to which the resonators are tuned exist as components of the sound, we

shall hear them singing out clearly above the general chorus of the other harmonics. Thus I have often successfully shown to an audience the composition of a composite sound.

(4) *Analysis by means of Resonant boxes carrying solid bodies tuned in unison with the sounds to which the air in these boxes resounds.*

This method is an excellent one when the composite sound can be obtained with intensity, when the boxes are accurately in tune with the solid bodies (forks or strings) attached to them, and when the boxes are in unison with the sonorous elements of the composite sound which we would analyze. The last-mentioned condition is not always fulfilled in the boxes on which forks have been some time mounted; for the former are apt to change their interior capacity by warping. This fact can readily be ascertained by partly introducing the hand into the mouth of the box and noting the effect on the intensity of the sound.

This method of analysis is similar to the one previously described; for the resonant box of a fork acts like a resonator, and can be used to intensify any harmonic of a composite sound. But there is an important difference in the methods; for the fork or the string, being in unison with the proper note of the mass of air in the box, is set in vibration by the latter; so that, after the box has been removed from the vicinity of the origin of the composite sound and the latter has ceased, we find that the fork sings out alone, and thus shows that it has selected from a chorus of harmonics that one which is in unison with its own tone. I have thus been able, by placing one fork after another of the series of the harmonics of the Ut_2 reed, to show the composition of its sound to a large audience with entire satisfaction. I have also succeeded with the following experiment. Forcibly sound the reed, and place around the opening of its "stump" all the eight forks of the harmonic series of Ut_2 , with the mouths of their resonant boxes toward the reed. After the reed has sounded for a few seconds, stop it; and we shall find that all the forks are in vibration, and thus singing together they approximately reproduce the sound of the reed. This experiment, to succeed, requires the resonant boxes, the forks, and the harmonics of the reed to be in exquisite unison.

(5) *Analysis by means of the beating of simple sounds of known pitch with the harmonics of the composite sound to be analyzed.*

If we use forks for the simple sounds, it will be better slightly to flatten or sharpen the note of the sound to be analyzed.

Then, knowing the number of beats that the fundamental of the note gives with its corresponding fork, we can designate the number of any other harmonic by the number of beats it makes with a fork of known pitch; for the number of beats observed, when referred to the number of beats of the fundamental as unity, will be directly as the number of the harmonic in the series. If we cannot well alter the pitch of the composite sound, then it will be well to use forks with sliding weights on graduated prongs, or loaded strings accurately tuned and provided with metre scales. In this system of analysis it is very important to guard against being led astray by the beating of resultant tones; therefore the forks or strings should be gently vibrated, and resonators used to assist the ear.

- (6) *Analysis by means of a loose membrane which receives the composite sonorous wave and transmits its vibrations through filaments or light rods to a series of forks mounted on their resonant cases.*

This method of analysis is the one we devised in our experimental confirmation of Fourier's theorem and described in section 1. We here wish to call attention to the precision of this method of analysis, while at the same time acknowledging the delicate instrumental conditions required to use it successfully. The principal interest attached to it is that it shows in a vivid manner the operation of the instantaneous decomposition of a composite wave into its elementary pendulum-vibrations. The method has also peculiar interest as showing, in the most striking manner, the exaltation of the action of very feeble periodic impulses to such degrees of intensity as to set into synchronous vibration very large masses of matter; and it may be well before we discuss the subject proper of this section to call attention to this very interesting result as set forth in the following experiment with our apparatus. To the membrane covering the hole in the box of the reed-pipe I attached one end of a fibre of silk-worm cocoon 1 metre long and weighing 1 milligram. The other end of the fibre was cemented to the face of a prong of an Ut_2 fork. This fork weighed 1500 grms., while the top of its resonant box and the air in the latter weighed respectively 102 and 22 grms. Therefore the fibre set in motion 1,624,000 times its own weight by only a fraction of the force which traversed it; and even this force was a yet more minute fraction of the whole energy of the ærial vibrations produced by the reed.

An experiment like the above is instructive as an analogical illustration of the manner in which we may imagine an ætherial vibration to produce chemical decomposition by causing such

powerful synchronous vibrations in the molecules of compounds as to shake their atoms asunder; for we have already seen how very feeble impulses sent through a medium of great tenuity can, when rapidly recurring and of the proper period, produce mechanical effects which at first sight appear incredible. Time is required in both cases to produce an appreciable action. The time required in the case of the sonorous vibrations decreases as their number per second increases; and in the case of the ætherial vibrations we have analogous phenomena. In the acoustical experiment, if the fork be much out of tune with the pulses transmitted by the fibre, no motion is produced in the fork; likewise we may imagine that when the period of vibration of one or more of the constituent atoms of a certain molecule is far removed from unison with any of the ætherial vibrations falling upon it, no motion or chemical decomposition will ensue.

The analogy between the two classes of phenomena is yet more striking when we remember that the fork *selects* from the composite vibratory motion which traverses the fibre only that vibration which is in unison (or only slightly removed from unison) with its own proper periodic motion; so likewise the molecule, or atoms of the molecule, select only those vibrations from the ray which are in unison with their own atomic periods; and on the tuning of the atom depends whether the result of the action of the ray will evince itself as heat, phosphorescence, chemical action, or fluorescence.

The following experiments show that the method of analysis we are now discussing surpasses in delicacy and sharpness of definition any other method in which sympathetically vibrating bodies are employed. As already shown, the forks select from the composite vibratory motion which strikes them only those simple vibrations which are in unison with their own vibratory periods. This remark, however, requires some modification, though the qualification necessary is less than is required when other similar methods of analysis are used. In all cases of covibration there is a certain range of pitch, above and below the sound which is in unison with the existing vibration, through which the covibrating body responds. The further the remove from unison the weaker the response*. But in some cases a slight remove of the pitch from unison will cause a great diminution in the intensity of the covibrations, while in others the same departure from unison causes only a slight or even inap-

* See Helmholtz's *Tonempfindungen*, p. 217, for the law connecting the variation of intensity of covibration with the variation of pitch in the exciting body.

preciable change in their intensity. For example, I connected the Ut_3 fork (the second in the harmonic series of Ut_2) to the membrane on the Ut_2 reed, and sounded the latter during a few seconds. After the reed was silent, I heard the fork sounding with intensity*. But on loading the fork with a piece of wax, so that it gave *five* beats per second with the note of the fork when unloaded, I could not, by any variation in the tension of the fibre or of any other circumstances of the experiment, set in vibration the fork by means of the pulses sent from the reed through the fibre; yet on placing the nipple of an Ut_3 resonator in my ear, I perceived that this flattened note of the fork produced a decided resonance—thus showing that although the fork could not respond to its own note flattened five beats per second, yet the resonator, under the same circumstances, did enter into sympathetic vibration. When the fork gave *four* beats per second it responded to the reed; but this response was only audible on placing the ear close to the mouth of the fork's resonant box. With *three* beats per second the sound of the fork was readily perceptible, while the resonator reinforced it very decidedly. When the fork was out of unison *two* beats per second, its sound was slightly increased; and with a departure of *one* beat per second the response of the fork was yet stronger, but greatly inferior in intensity to that produced when the fork was in unison with its proper sound (the second harmonic of the reed); yet the resonator reinforces this flattened sound as forcibly as it does that which emanates from the unloaded fork. These facts concerning the want of sharpness in the detection of pitch by means of resonators are not in accordance with the statements made in recent popular works on sound, where the resonator is described as remaining dumb until the exact pitch to which it is tuned is reached, when it responds with a suddenness which has been compared to an explosion!

The fork, the stretched fibre, and the intensity of the sound of the reed remaining in the same conditions as in the above experiments, I gradually unloaded the fork until it made only *one beat in eight seconds*; and yet even this slight departure from unison with the second harmonic of the reed was evident in the difference in the intensities of the fork's responses when thus

* It is perhaps hardly necessary to state that in all these experiments I first ascertained that when the fibre was detached from the fork, the latter did not emit a sound caused by the action of the intervening vibrating air. This condition is readily attained by screening the mouths of the resonant boxes, or by turning these mouths away from the sounding reed. Also I should here state that the reed was tuned to the fork after the fibre was stretched between the latter and the membrane on the reed-pipe.

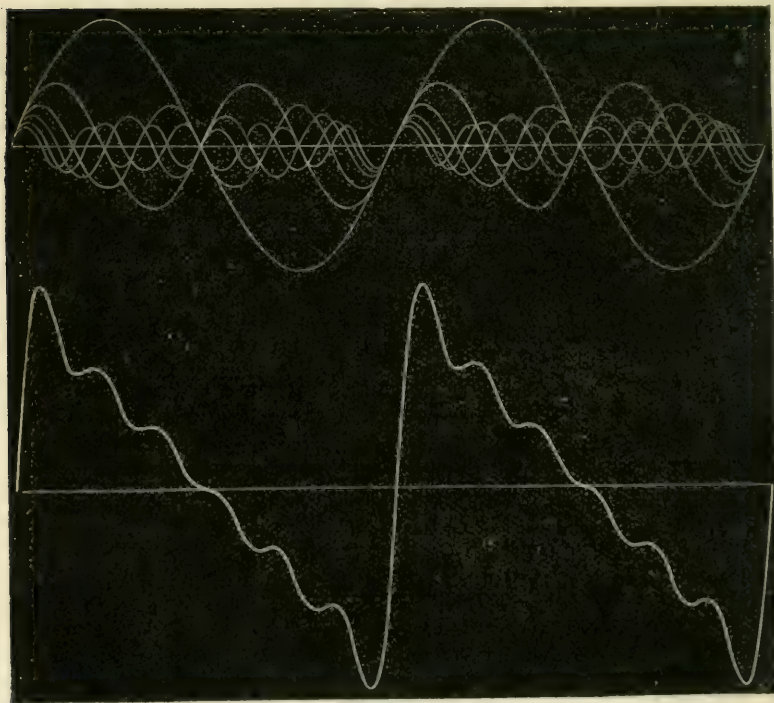
loaded and when the wax was removed. This fact I have repeatedly confirmed by testing the intensities of the two sounds by different hearers who were placed so that they could not see when the fork was loaded or unloaded. Now E. H. Weber has found that only the most accomplished musical ears can distinguish between the pitch of two notes whose vibrations are as 1000 to 1001; but by the above method we can readily detect a departure from unison in the two notes amounting to the interval of 2000 to 2001, or to the $\frac{1}{128}$ of a semitone.

In connexion with the preceding observations the following experiments on resonators and sympathetic vibrations may be of interest. I substituted for the ear the manometric flames of König, viewed in a revolving mirror, and tested the response of the resonators to sounds not in accord with their proper notes. The results agreed with those previously obtained on placing the resonator to the ear. I now mounted an Ut_3 fork on its resonant case; and sounding it *strongly* before all the resonators of the harmonic series of Ut_2 , I caused all the manometric flames connected with these resonators to vibrate, each giving the same number of serrations as when the Ut_3 fork was brought near its own resonator. The same result was obtained when the fork was separated from its case, with this important difference, however, that when the face of fork Ut_3 was brought near the mouth of the Ut_4 resonator, the flame connected with this resonator gave at the same time serrations belonging both to Ut_3 and to Ut_4 ; and this result accords with the following experiment. If one sounds the Ut_3 fork on its box, and brings the mouth of the latter near the mouth of the box of the Ut_4 fork, the Ut_4 fork will covibrate; and after Ut_3 has ceased to vibrate, Ut_4 will sound out quite clearly. If, however, Ut_4 be lowered in tone by weighting it with a piece of wax so that it gives two beats per second with its proper tone, then the Ut_4 fork cannot be set into sympathetic vibration by impulses from Ut_3 . Also if forks Ut_3 and Ut_4 be detached from their boxes and fork Ut_3 be strongly vibrated while the face of one of its prongs remains quite close and parallel to the face of a prong of fork Ut_4 , the latter is set in vibration by the impulses of Ut_3 sent through the intervening air. Fork Ut_3 was now loaded so that it successively made 1, 2, 3, and 4 beats per second with the true Ut_3 pitch. When it made 3 beats per second, it caused Ut_4 to vibrate so feebly as to be barely audible, while 4 beats per second departure of Ut_3 from unison produced no effect on Ut_4 .

The following experiment was now made to show the want of precision in the determination of the exact pitch of sonorous

elements by means of resonators. We have just seen that the Ut_3 fork could not vibrate the Ut_4 fork when both forks were on their cases and when Ut_3 was flattened by 2 beats per second, and also that a departure of 4 beats per second prevented Ut_3 from setting Ut_4 in sympathetic vibration when both forks were off their resonant cases and with their prongs close and parallel to each other. But when the Ut_3 fork was loaded so that it made from 15 to 20 beats with its proper tone, it caused the serrations of the Ut_3 resonator to appear accompanied by the serrations of its octave. The Mi_3 fork, although it developed the serrations belonging to its own pitch when brought opposite the Ut_3 resonator, yet did not, as might have been expected, develop its own tone and the octave when brought near the mouth of the Ut_4 resonator. It is probably not necessary to add that the above effects of simultaneously

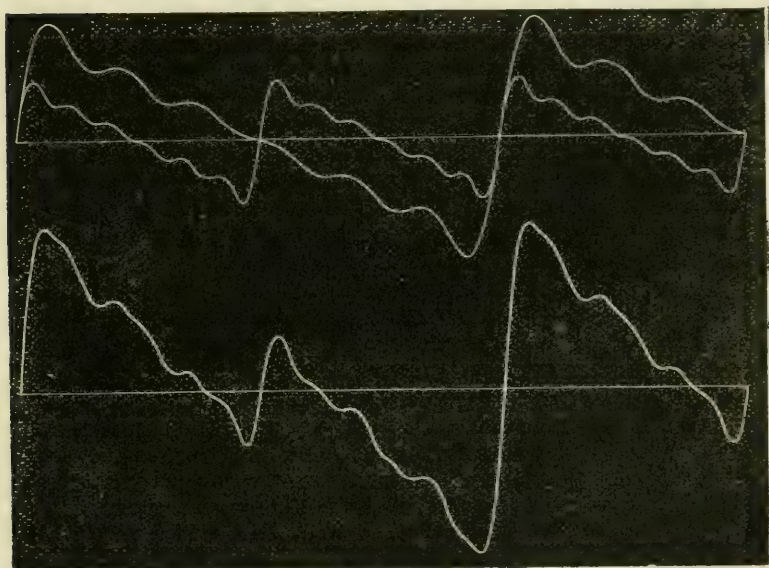
Fig. 3.



Curve of a musical note, being the resultant of the simple vibrations of its first six harmonics.

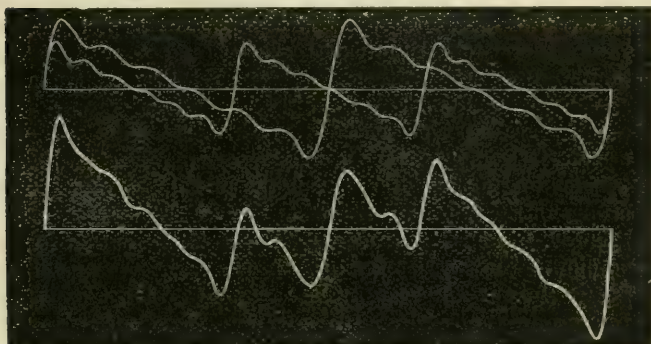
developing in the flame the serrations of the proper note of a resonator and those of its octave are only produced when the fundamental sound is intense.

Fig. 4.



Resultant curve, formed by combining the curve of a musical note with that of its octave. $\lambda : \lambda' :: 1 : \frac{1}{2}$.

Fig. 5.



Resultant curve, formed by combining the curve of a musical note with that of its fifth. $\lambda : \lambda' :: 1 : \frac{2}{3}$.

6. *The Curve of a Musical Note formed by combining the sinusoids of its first six harmonics, and the curves formed by combining the curves of musical notes corresponding to various consonant intervals.*

We have already seen that any composite vibration which produces in us the sensation of a musical note, can always be reproduced by the simultaneous production of a certain number of the simple sounds of an harmonic series, provided these simple sounds have the proper relative intensities. Therefore, to obtain the resultant curve corresponding to a musical note, we draw on one axis its harmonic components with their proper wave-lengths and amplitudes; and the algebraic sums of their corresponding ordinates are the ordinates of the required resultant curve.

Fig. 3 is the curve of a musical note, being the resultant of the simple vibrations of its first six harmonics. The first six harmonics having been drawn on a common axis, I erected five hundred equidistant ordinates, and extended these ordinates some distance below the axis on which I desired to construct the resultant. The algebraic sum of the ordinates, passing through the harmonic curves, were transferred to the corresponding ordinates of the lower axis; and by drawing a continuous line through these points, I formed the resultant curve. The first six harmonics are alone used in the combinations which I have given, because the seventh, ninth, eleventh harmonics, and the major number of those above the twelfth, form dissonant combinations with the lower and more powerful harmonics. Indeed the harmonics above the sixth are purposely eliminated from the notes of the piano by striking the string in the neighbourhood of its seventh nodal point. The amplitudes of the harmonics of fig. 3 are made to vary as the wave-length—not that this variation represents the general relative intensities in such a composite sound, but they were so made to bring out strongly the characteristic flexures of the resultant. To simplify the consideration of the curves, they are all represented with the same phase of initial vibration. Of course the resultants have an infinite variety of form, depending on the difference in the initial phase and on the amplitudes of the harmonic elements.

In figs. 4, 5, and 6 I have drawn the resultant curves formed by combining the curves of musical notes corresponding to the various consonant intervals indicated below the curves. As these curves are the resultants formed by the combination of the composite vibrations of musical notes, it follows that the components of these curves are not simple harmonics, as in the case of fig. 3, but are derived from the *resultant* of fig. 3 by reducing

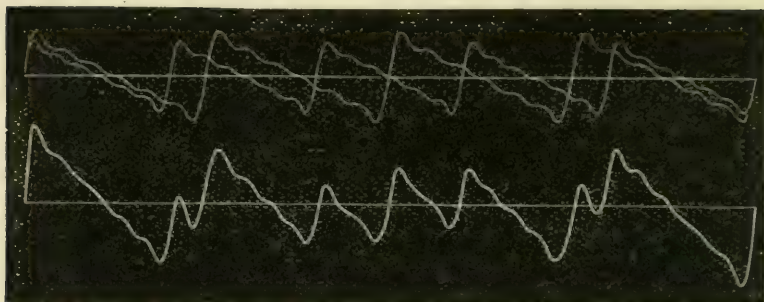
to one fourth the amplitude of that curve and by taking wavelengths corresponding to the intervals indicated below the figures.

All the curves which I have given in this paper were first drawn on a large scale and then reduced by photography to a size suitable to be transferred to the engraver's block.

7. *Experiments in which are produced from the above curves (Section 6) the Motions of a Molecule of Air when it is animated with the resultant action of the six elementary vibrations forming a musical note, or is set in motion by the combined action of sonorous vibrations forming various musical intervals.*

We may imagine the curve corresponding to a musical note, represented in fig. 3, as formed by the trace of a vibrating molecule of air, or of a point of the tympanic membrane, on a surface which moves near these points. Therefore, if we slide this curve along its axis under a slit in a screen which allows only one point of the curve to appear at once, we shall reproduce in this slit the vibratory motion of the aerial molecule and of the point on the tympanic membrane. I have exhibited this motion in a continuous or, rather, recurring manner as follows:—On a piece of Bristol board I drew a circle, and in one quadrant of this circle I drew five hundred equidistant radii. On these radii, as ordinates, I transferred the corresponding values of the same ordinates of the resultant of fig. 3, diminished to one fourth of their lengths. I thus deflected the axis of curve, fig. 3, into one fourth of a circle-curve; and this repeated four times on the Bristol board, rendered the curve continuous and four times recurring, as shown in fig. 7. I now cut this curved figure out of the board and used it as a template. I placed the latter centred on a glass disk 20 inches in diameter. The disk was covered on one side with opaque black varnish; and with the template and the separated points of a pair of spring dividers I removed from the glass disk a sinuous band, as shown in fig. 7. The glass disk was now mounted on a horizontal axis and placed in front of a lantern the diameter of whose condensing-lens was somewhat greater than the amplitude of the curve. The image of that portion of the curve which was in front of the condenser was now projected on a screen; and then a piece of cardboard having a narrow slit cut in it was placed close to the disk with the slit in the direction of one of its radii. On now revolving the disk I reproduced on the screen the vibratory motion of a molecule of air, or of a point on the tympanic membrane, when these are acted on by the joint impulses of the first six harmonic or pendulum-vibrations forming a musical note. On slowly

Fig. 6.



Resultant curve, formed by combining the curve of a musical note with that of its major third. $\lambda : \lambda' :: 1 : \frac{4}{5}$.

Fig. 7.



rotating the disk, one can readily follow the compound vibratory motion of the spot of light; but on a rapid revolution of the disk, persistence of visual impressions causes the spot to appear lengthened into a band; but this band is not equally

illuminated; it has six distinct bright spots in it, beautifully showing the six inflections in the curve.

By sticking a pin in the centre of fig. 7 as an axis about which revolves a piece of paper with a fine slit, the reader can gain some idea of the complex motion which I have described.

From the curves of figs. 4, 5, and 6 can similarly be reproduced their generating motions. Of course it is understood that in all these cases the amplitudes of these vibrations are enormously magnified when compared with the wave-lengths, and that it is really only when the amplitudes of the elementary pendulum-vibrations are infinitely small that the resultant curves we have given can be rigorously taken as representing what they purport to represent; for the law of the "superposition of displacements" depends on the condition that the force with which a molecule returns to its position of equilibrium is directly proportional to the amount of displacement, and this condition only exists in the case of infinitely small displacements; yet the law holds good for the majority of the phenomena of sound.

As a periodic vibration can alone produce in the ear the sensation of sound, and as the duration of the period is always equal to the least common multiple of the periods of the pendulum-vibrations of the components, it follows that in the case of a simple sound, or of a sound formed of an harmonic series, the period equals the time of one vibration of the fundamental; but in the case of any other combinations the duration of the period of the recurring vibration increases with the complexity of the ratio of the times of vibration of the components. Thus the durations of the following combinations are placed after them in fractions of a second—

$$C_3 + C_4 = \frac{1}{2 \cdot 5 \cdot 6}, \quad C_3 + G_3 = \frac{1}{1 \cdot 2 \cdot 8}, \quad C_3 + E_3 = \frac{1}{6 \cdot 4}, \quad C_3 + E_3 + G_3 = \frac{1}{2 \cdot 5 \cdot 6}, \\ C_3 + E_3 + G_3 + C_4 = \frac{1}{1 \cdot 7} \text{ of a second.}$$

The above-mentioned facts suggest a curious physiological inquiry, viz. Does it require a combination of sounds, simple or composite, to remain on the ear the duration of an entire period in order that it shall give the same sensation as is produced when the same combination is sounded continuously? In other words, will a portion of the recurring composite vibration produce the same sensation as an entire period or several periods? The solution of this problem has been the object of a prolonged experimental research; but up to this time the results have been so difficult of interpretation that I have not arrived at any certain knowledge on the subject. I shall, however, return to this interesting but very difficult work.

Stevens Institute, Hoboken, N. Jersey, U.S.A.

LXX. *On an Absolute Galvanometer.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE description which I gave in the October Number of your Magazine of an absolute galvanometer has given rise to a communication in the November Number of the *Journal de Physique théorique et appliquée*, by Professor A. Lallemand. Professor Lallemand states that the instrument described by me is identical with the electrodynamic torsion-balance described by him in the years 1848 and 1851 in the *Annales de Chimie et de Physique*. At the same time Professor Lallemand describes the more recent modifications which he has introduced into his instrument.

It becomes me, therefore, briefly to notice (1) the similarity between Professor Lallemand's electrodynamic torsion-balance as described in the *Annales de Chimie et de Physique* and my absolute galvanometer, and (2) between the latter and Professor Lallemand's most recent instrument.

The essential difference between Professor Lallemand's balance and my galvanometer is this, that in the former the current-strength is measured by the repulsion between flat opposed spirals through which the current passes; in the latter, repulsion is exerted between electromagnets excited by the current. The law of comparative repulsion is the same; but I believe that the former method (which has been, since Lallemand described his instrument, much employed for the determination of electrodynamic constants) cannot give such delicate results as when the form I suggest is adopted.

In the modification of his balance which Professor Lallemand now describes, he proposes to employ independently excited electromagnets to repel movable solenoids through which the current under examination passes, or to repel the latter by means of semicircular permanent magnets. These latter modifications no doubt increase the similarity between our instruments. But I cannot in any sense admit that the absolute galvanometer lately described by me is identical with the electrodynamic balance described by Lallemand twenty years ago. The latter in all its forms is admirably adapted for the measurement of induced currents. I believe, under correction, the former is more adapted for the absolute comparison of currents, especially of weak ones.

Your obedient Servant,

FREDERICK GUTHRIE.

LXXI. *Notices respecting New Books.*

An Elementary Exposition of the Doctrine of Energy. By D. D. HEATH, M.A., formerly Fellow of Trinity College, Cambridge. London: Longmans, Green, and Co. 1874 (post 8vo, pp. 129).

WE have here a very clear and exact “Elementary Exposition of the Doctrine of Energy.” The author presupposes on the part of the reader an acquaintance with the elements of Mechanical Science (which is, indeed, an indispensable preliminary), and proceeds, after a brief introduction, to expound the Doctrine of Energy of Motion and Energy of Position, *i. e.* the principle of *vis viva* embodied in the formula

$$\Sigma(mv^2) = C + 2 \int \Sigma m(Xdx + Ydy + Zdz),$$

understood with the usual well-known restrictions. This occupies more than half of the volume (pp. 1–69). The author begins from the most elementary principles, and takes great care to notice the conditions presupposed in the various statements as they follow one after another—thus giving an account of the subject which might be read with advantage even by an advanced student. There is, of course, nothing new in the leading facts; but the author has thoroughly mastered the subject for himself, and gives a lucid account of it as it looks from his own point of view; and in this sense it may be justly spoken of as an original exposition of the matter.

From the dynamical doctrine the author passes to the consideration of the subjects of impact, friction, and heat, of the energy derived from heat, and of the Energy of Elastic Fluids; these he treats at some length, and then notices more briefly Electric Energy, Chemical Energy, Animal Energy, Energy of Vegetation, and Dissipation of Energy. Of these, Chemical Energy is the only form of Energy which the author notices with any thing approaching to detail—though, in this as in other cases, strictly under the view of the connexion between Chemical Change and Energy considered as a “numerically measurable thing.” This, properly speaking, completes the subject; but a sort of Appendix of sixteen pages is added on “Molecular Theories.” The whole subject is treated with the same sort of originality which we noted above as exemplified in the account of the principle of *vis viva*; and the work (for this reason) might be read with advantage by almost any student; but it will be no more than right to let the author speak for himself as to the class of readers for which it is specially designed. He says:—“My aim was to lay before them [*i. e.* the boys of the sixth form in the Surrey County School], lads of from sixteen to eighteen, soon about to quit school for active life, not a popular account of recent discoveries or theories, . . . but such an exhibition of the principles and methods of modern science, and of some of the results obtained therefrom, as might show them the use of the mathematical groundwork in which they had been carefully instructed,

and perhaps induce some of them to pursue their own scientific education after leaving school."

The book, as a whole, has been carefully drawn up; but here and there are traces of a want of skill in the mechanical part of book-making: *e. g.* on p. 121 there is a mysterious and unexplained diagram; it looks like an egg arrested on its way through a tube contracted in the middle. What it really means we cannot say; but we suppose it is intended to illustrate the difference between the orbits of a planet and a comet, though the sides of the contracted tube are not in the least like a hyperbola. We must also demur to the statement on the same page, that the fact that "two bodies subject only to their mutual attractions . . . will move round one another in some curved orbit," is Newton's First Proposition; that proposition concerns an "immobile centrum."

Again, in the article on Chemical Energy, the author gives a careful account of the action of a galvanic cell, explaining the principal points and the meaning of the technical terms as he goes on; *e. g.* he explains what is meant by "closing a circuit;" but suddenly the reader comes upon the statement that a "sufficient number of such cells will work and decompose water in the *electrolytic cell*" (p. 102). What the "electrolytic cell" is he must find out for himself from the context.

Another point may be noticed; and it is a fault (to speak bluntly) which is commonly to be found in books of science: we mean that of sticking in here and there arbitrarily chosen historical notices. The research required for a correct exposition of the principles of a given science is very different from that required for giving a correct account of the history of its formation. A writer who merely expounds principles is under no obligation to go into the history of his subject; but if he do, the main points should be noticed with some degree of completeness; or if he restrict himself to one or two points, it should be because their history is not well known, and his researches enable him to throw light upon them. Mr. Heath adopts the more usual course: his historical notices are put in quite arbitrarily, and the longest of them runs off into a conjecture which is quite a curiosity in its way. "The very oldest speculation on Physical Science that we know of is the Doctrine of Thales of Miletus, about 600 B.C., that water is the substance of all things. Perhaps, could we cross-examine him, his essential meaning might be found to be that all things can, like water, exist in the fluid, the solid, and the *aëriform*, vaporous, or gaseous state" (p. 78). We must own that we should be a great deal surprised if such were in fact the result of the cross-examination; at all events we incline to follow Mr. Grote, who tells us that "Aristotle says little about Thales, and that little in a tone of so much doubt that we can hardly confide in the opinions and discoveries ascribed to him by others" (Plato, vol. i. p. 4).

Transits of Venus. A Popular Account of Past and Coming Transits.

By RICHARD A. PROCTOR, *B.A. Camb.* London: Longmans and Co.

The Approaching Transit of Venus. By WILLIAM HUGGINS, *D.C.L.,*

LL.D., F.R.S. Manchester and London: Heywood and Pitman.

The first transit of Venus of the present century has now passed; the observations, wherever practicable, have been made; and we are anxiously waiting for the publication of the results, which must necessarily occupy some time in preparation. Just on the eve of its occurrence, and in our opinion somewhat late, one of the best-written of Mr. Proctor's works in some important respects appeared; and about the same time Dr. Huggins delivered a lecture on the transit at the Town Hall, Hulme, near Manchester, which has been incorporated in the sixth series of "Science Lectures for the People." The most interesting part of Mr. Proctor's work is, without doubt, Chapter IV., entitled "Transits and their Conditions," extending from p. 93 to p. 155. The greatest fault we find with it is a little prolixity in the treatment; otherwise it is the most scientific of his writings which we have read. We lose, it is true, the raciness of the sensational character appertaining to some of his lighter essays; but, on the other hand, we rejoice that he has given us the conditions of transits in so clear, intelligible, and simple a manner, illustrated, as they are, by several excellent diagrams. We should strongly recommend that this chapter be read first as an advantageous preparation for perusing the historical account of the past transits in 1639, 1761, and 1769 in Chapters I., II., and III.

Had we not felt it imperative to read the last chapter, we should most willingly have closed the book, as indications presented themselves of the controversial bitterness which has characterized more or less the writings of Mr. Proctor on the transits of Venus. Having read this chapter, we rise from its perusal with a conviction that one of the best-written of Mr. Proctor's works has been marred by a reference to that which at most is but a matter of opinion. Had the author contented himself with a notice of the preparations for the transit of December 9 in every part of the world, his book would have been all that could have been desired. It would appear from a letter of Mr. Proctor's to a contemporary, that he has been driven to the course adopted from a desire to accede to the wishes of those from whom it has been his misfortune to differ. He says, "I have therefore, though with regret, given a few pages to the subject in my book on Past and Coming Transits. Those of my readers who think as I do, that we have had more than enough of the subject, will do well to leave uncut pp. 180 to 198 of that work. I hope that circumstances will permit me to remove this portion altogether from future editions; for, to say the truth, the subject is not particularly pleasant, and I would much rather be allowed to say nothing more about it." We sincerely hope that Mr. Proctor will submit to the prompt-

ings of his better-informed judgment, and in his next edition leave out these objectionable passages; and we feel assured that his scientific reputation will be enhanced in consequence. The following passage from the close of the volume expresses, as we think, all that was necessary for Mr. Proctor to say in such a work as the present:—"I think the astronomers of the first years of the twenty-first century, looking back over the long transitless period which will then have passed, will understand the anxiety of astronomers in our own time to utilize to the full whatever opportunities the coming transits may afford; and I venture to hope that should there then be found among old volumes on their book-stalls the essays and charts by which I have endeavoured to aid in securing that end (perhaps even this little book in which I record the history of the matter), they will not be disposed to judge over harshly what some in our own day may have regarded as an excess of zeal."

It gives us the greatest pleasure to speak in the highest terms of the lecture by Dr. Huggins. Every important particular relative to the distance of the sun is to be found in it expressed in simple and intelligible language; and he has further mentioned a point not found in Proctor's book, viz. the use of the spectroscope in observing the approach of Venus to the sun's limb. With the exceptions above named, we most cordially recommend both works to our astronomical readers as valuable contributions to the literature of the solar system—Proctor's as particularly adapted to the student, Huggins's as giving a condensed but popular view of the subject.

LXXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 474.]

April 23, 1874.—Joseph Dalton Hooker, C.B., President, in the Chair.

THE following communication was read:—

"On the Refraction of Sound by the Atmosphere." By Prof. Osborne Reynolds, Owens College, Manchester.

The principal object of this paper is to show that sound is refracted upwards by the atmosphere in direct proportion to the upward diminution of the temperature, and hence to explain several phenomena of sound, and particularly the results of Prof. Tyndall's recent observations off the South Foreland.

The paper commences by describing the explanation of the effect of wind upon sound, viz. that this effect is due to the lifting of the sound from the ground, and not to its destruction, as is generally supposed.

The lifting of the sound is shown to be due to the different velocities with which the air moves at the ground and at an eleva-

tion above it. During a wind the air moves faster above than below, therefore sound moving against the wind moves faster below than above, the effect of which is to refract or turn the sound upwards; so that the "*rays*" of sound, which would otherwise move horizontally along the ground, actually move upwards in circular or more nearly hyperbolic paths, and thus, if there is sufficient distance, pass over the observer's head. This explanation was propounded by Prof. Stokes in 1857, but was discovered independently by the author.

The paper then contains the description of experiments made with a view to establish this explanation, and from which it appears that:—

1. The velocity of wind over grass differs by one half at elevations of 1 and 8 feet, and by somewhat less over snow.

2. When there is no wind, sound proceeding over a rough surface is destroyed at the surface, and is thus less intense below than above.

3. That sounds proceeding against the wind are lifted up off the ground, and hence the range is diminished at low elevations; but that the sound is not destroyed, and may be heard from positions sufficiently elevated with even greater distinctness than at the same distances with the wind.

4. That sounds proceeding with the wind are brought down to the ground in such a manner as to counterbalance the effect of the rough surface (2); and hence, contrary to the experiments of Delaroché, the range at the ground is greater with the wind than at right angles to its direction, or where there is no wind.

On one occasion it was found that the sound could be heard 360 yards with the wind at all elevations, whereas it could be heard only 200 yards at right angles to the wind, standing up; and, against the wind, it was lost at 30 yards at the ground, 70 yards standing up, and at 160 yards at an elevation of 30 feet, although it could be heard distinctly at this latter point a few feet higher.

As might be expected, the effect of raising the bell was to extend its range to windward, to even a greater extent than was obtained by an equal elevation of the observer.

These results agree so well with what might be expected from the theory as to place its truth and completeness beyond question.

It is thus argued that, since the wind raises the sound so that it cannot be heard at the ground, by causing it to move faster below than above, any other cause which produces such a difference in velocity will lift the sound in the same way; and therefore that an upward diminution in the temperature of the air must produce this effect; for every degree of temperature between 32° and 70° adds nearly one foot per second to the velocity of sound. Mr. Glaisher's balloon observations* show that when the sun is shining with a clear sky, the variation from the surface is 1° for every hundred feet, and that with a cloudy sky $0^{\circ}5$, or half what it is with a clear

* Brit. Assoc. Report, 1862, p. 462.

sky. Hence it is shown that “rays” of sound, otherwise horizontal, will be refracted upwards in the form of circles, the radii of which are 110,000 feet with a clear sky, and 220,000 with a cloudy sky—that is to say, the refraction on bright hot days will be double what it is on dull days, and still more under exceptional circumstances, and comparing day with night.

It is then shown by calculation that the greatest refraction (110,000 radius) is sufficient to render sound, from a cliff 235 feet high, inaudible on the deck of a ship at $1\frac{3}{4}$ mile, except such sound as might reach the observer by divergence from the waves passing over his head; whereas, when the refraction is least (220,000 radius), that is, when the sky is cloudy, the range would be extended to $2\frac{1}{2}$ miles, with a similar extension for the diverging waves, and under exceptional circumstances the extension would be much greater. It is hence inferred that the phenomenon which Prof. Tyndall observed on the 3rd of July and other days (namely, that when the air was still and the sun was hot he could not hear guns and other sounds from the cliffs 235 feet high more than 2 miles, whereas when the sky clouded the range of the sounds was extended to 3 miles, and, as evening approached, much further) was due, not to the stoppage or reflection of the sound by clouds of invisible vapour, as Prof. Tyndall has supposed, but to the sounds being lifted over his head by refraction in the manner described; and that, had he been able to ascend 30 feet up the mast, he might at any time have extended the range of the sounds by a quarter of a mile at least.

April 30.—Prof. Andrew Crombie Ramsay, LL.D., Vice-President, in the Chair.

The following communication was read :—

“On the Improvement of the Spectroscope.” By Thomas Grubb, F.R.S.

The importance, as an instrument of research, which the spectroscope has reached within a few years, renders any improvement therein a matter of general scientific interest. Hitherto it has been under a disadvantage, which, though slight in amount in those cases in which the dispersive power of the instrument is moderate, becomes a rather serious annoyance to the observer when a number of prisms are used in serial combination, and the curvature of the spectral lines is proportionally increased, and only to be restrained in appearance by using a narrow breadth of the spectrum.

I have lately thought of a very simple and practical remedy (which may indeed have occurred to others, but which I have not seen mentioned), whereby those lines are rendered palpably straight in a very large field; but previous to describing it, it is desirable to refer to a statement appearing in the ‘Astronomical Notices’ for last month (March), viz. that the spectral lines can

be rendered perfectly straight simply by returning them (after their first passage through a series of prisms arranged for minimum deviation) by a direct reflection from a plane mirror; and, further, that this has been accomplished in a spectroscope in construction for the Royal Observatory.

Such a statement has, as might be expected, produced several inquiries; in one case the querist is much interested, viz. by having a very large spectroscope in hand which, from its construction, involves the question of straight or curved lines resulting. It therefore seems desirable to remove any illusion which may be entertained, by a short consideration of the economy of the spectroscope, so far as the question of curvature is concerned.

The curvature of the spectral lines may be considered a function of the dispersion of a prism; it (the curvature) not only always accompanies the dispersion, but, further, its character is always the same with respect to the dispersion—that is to say, the centre of curvature will be found invariably to lie in the same direction with respect to the direction of the dispersion, the lines being invariably concave towards that end of the spectrum having the more refrangible rays*. This (which admits of the clearest proof) is adequate to show the impossibility that, by any kind of inversion, whether by reflections or otherwise, we can neutralize the curvature while doubling the dispersion.

If we examine the spectrum, as produced by a series of prisms placed in the position of minimum deviation, we necessarily find that the lines of higher refrangibility, also their centres of curvature, lie towards the centre of the polygon which the prisms themselves affect; and if we arrest the rays at any part of the circuit, and reflect them directly back by a plane mirror, this reflection reverses (right for left) not only the direction of the centre of curvature of the lines, but also the direction of the spectrum itself, both which are consequently doubled in amount after the rays have performed the second, or return, passage through the prisms; or (conversely) if, after the first passage through the prisms, we reflect the rays so as to pass through a similar set in such manner as to neutralize the curvature of the first set, we shall find the resulting dispersion reduced to zero.

The writer of the article having alluded to a difference between the reflection as given by a plane mirror and a prism of (double) total reflection, it may be observed that, so far as the dispersion and curvature are concerned, the cases are practically identical,

* Professor Stokes has indeed investigated a form of compound prism in which the resulting lines are straight, and on the same principle we may combine prisms (using of course media of different optical powers) in which, with a balance of dispersion remaining, the curvature might be found reversed; but this does not affect the general law. The curvature in that compound prism (which was the result of various trials, and first used in the spectroscope of the Great Melbourne Telescope, and now, I apprehend, in pretty general estimation and use) probably has a less proportional curvature of the lines than the simple prism.

the difference being that, in the double reflection, there is a *vertical* inversion of the spectrum, which, however, produces no discernible effect in either the spectrum or curvature of the lines; and as the spectroscope constructed with the double reflecting prism is known to produce, with double dispersion, double curvature, we here have an additional proof, if such were required, that the single reflecting mirror does the same.

The remedy, or means of producing straight spectral lines, which I have alluded to, is simply that of constructing the "slit" with curved instead of rectilinear edges. There is but little practical difficulty incurred in construction, and no apparent objection to its use. It may be objected that for each variation of prism-power in use there should be a special slit. It is, however, only in spectroscopes arranged for high dispersion that the curvature becomes objectionable; in such there is seldom a change required, and a single slit of medium balancing-power would probably remove all practical difficulty, or objectionable curvature of the lines. I have found by trial that, when two compound prisms were in use, giving a dispersion from A to H of nearly 14° , the spectral lines were straight in a field of one degree, when the radius of curvature of the slit was made 1.25 inch.

[*Note on the above Paper.*]

If a ray of light be refracted in any manner through any number of prisms arranged as in a spectroscope, undergoing, it may be, any number of intermediate reflections at surfaces parallel to the common direction of the edges of the prisms—or, more generally, if a ray be thus refracted or reflected at the surfaces of any number of media bounded by cylindrical surfaces in the most general sense (including, of course, plane as a particular case), the generating lines of which are parallel, and for brevity's sake will be supposed vertical, and if α be the altitude of the ray in air, α' , α'' , . . . , its altitudes in the media of which the refractive indices are μ' , μ'' , . . . , then

- (1) The successive altitudes will be determined by the equations

$$\sin \alpha = \mu', \sin \alpha' = \mu'', \sin \alpha'' = \dots,$$

just as if the ray passed through a set of parallel plates.

- (2) The course of the horizontal projection of the ray will be the same as would be that of an actual ray passing through a set of media of refractive indices $\frac{\mu' \cos \alpha'}{\cos \alpha}$, $\frac{\mu'' \cos \alpha''}{\cos \alpha}$, . . . instead of μ' , μ'' , As $\alpha' < \alpha$, the fictitious index is greater than the actual, and therefore the deviation of the projection is increased by obliquity.

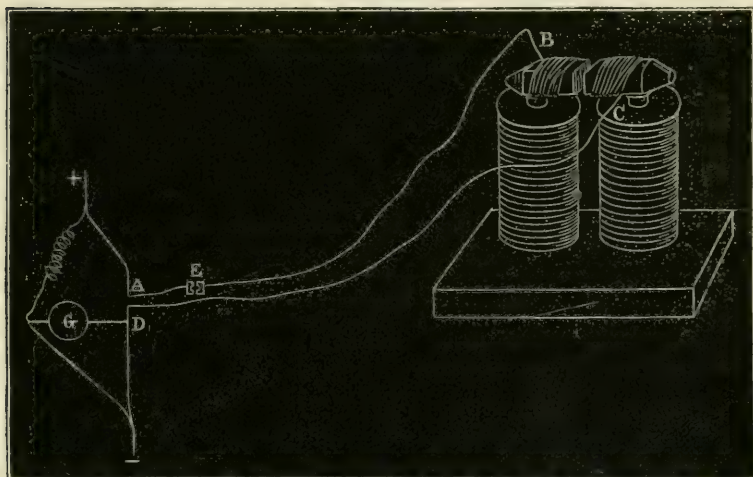
These two propositions, belonging to common optics, place the justice of Mr. Grubb's conclusions in a clear light.—April 30, G. G. STOKES.]

May 7.—William Spottiswoode, M.A., Treasurer and Vice-President, in the Chair.

The following communication was read:—

“Preliminary Experiments on a Magnetized Copper Wire.” By Professor Balfour Stewart, LL.D., F.R.S., and Arthur Schuster, Ph.D.

1. The following experiments were made in the Physical Laboratory of Owens College, Manchester. The copper wire employed (A B C D, see fig.) was found to contain no perceptible trace of



iron, nor was it sensibly magnetic, behaving quite in a neutral manner when tested by the highest magnetic power at our disposal. It was covered with gutta percha. The diameter of the wire was 0.0487 inch. The wire was wound fifty-three times, in one direction, round the poles of a powerful electromagnet, the length of wire encircling these poles being about twelve metres. The direct distance of the magnet from the galvanometer, G, was about twelve metres.

A Wheatstone bridge was employed, and a very delicate Thomson's reflecting galvanometer by Elliott Brothers, of which the resistance was 5540 B.-A. units. A circuit-breaker was placed in the circuit at E, close to the bridge. On some occasions, we used one consisting of a solid key, which might be removed, thus breaking the circuit; but, on other occasions, a fluid or mercurial circuit-breaker was employed.

When the left-hand pole of the electromagnet (see fig.) was made north the arrangement was called (1), and when the other pole was made north the arrangement was called (2). It will thus be seen,

from the figure, that the current went round the magnet in the same direction as the molecular currents of arrangement (2).

Experiments were made at intervals of two minutes; and, on each occasion, the current was allowed to pass through the bridge for ten seconds, the measurement being taken by the first swing of the galvanometer, which lasted for about eight seconds. Three cells of Grove's battery were used for producing this current; but, on the other hand, six similar cells were employed for magnetizing the electromagnet. The arrangements for magnetizing are not shown in the figure. The distance of the magnet was too great to affect the galvanometer-needle so as to alter its sensibility, the average deflection causing a difference in the zero of about four divisions of the scale.

2. In the first experiments made, the key at E (see fig.) was not taken out before the magnetism was put on or off, in consequence of which the induction-current, due to the wire coiled round the magnet, affected the galvanometer on these occasions; but, after December 12th, the key was taken out, so that no induction-current passed.

The following is a specimen of the observations made :—

December 17, 1873.

Time of putting on current.	Whole deflection observed (increasing deflection denotes increasing resistance in A B C D).	Condition of magnet.
11 ^h 11 ^m	312	off
13	317	off
15	311	off
17	345	(1)
19	328	off
21	306	(1)
23	303	off
25	293	(1)
27	300	off
29	290	(1)
31	307	off
33	283	(1)
35	292	off
37	288	(1)
39	302	off
41	292	(1)
43	309	off

It will be seen from this experiment that the *first effect* of putting on the magnetism was a marked increase of resistance; but, with this exception, the resistance, when the magnetism was on, was less than the mean of the two resistances on both sides of it, representing the magnetism off.

3. The arrangement remained untouched, as far as we know, from December 15, when it was finally made, until December 19,

when the experiments were interrupted during the Christmas holidays; and in all cases the *first effect* of putting on the magnetism was a marked increase of resistance. For instance, we have—

Date.	First off.	On first effect.	Second off.
Dec. 16	0	+36 on (2)	+ 3
„ 17	0	+34 on (1)	+17
„ 18	0	+54 on (1)	+24
„ 19	0	+33 on (1)	-18

It was soon seen that this *first effect* had some reference to the time elapsing since the last experiments were made. For instance, in the above Table, we see for December 18th a marked increase of resistance when the magnet was first put on; but, on the afternoon of that day, the experiments were repeated, and there was no apparent increase of resistance in this *first effect*. Next, with regard to the *average effect*: on Dec. 16th, 17th, and 18th, this *average effect* of magnetism was a decrease of resistance; but on Dec. 19th there was an apparent increase of resistance when the magnetism was on. We cannot say that nothing had been done to the arrangement between the 18th and 19th of December that might account for this change; but whatever was done must have escaped our recollection. Undoubtedly a good many experiments were made during the time between the 15th and 19th of December, and the direction of the magnetism was frequently changed. This curious anomaly, occurring unexpectedly, induced us to limit our future experiments to a definite set each day.

4. The experiments were resumed on January 7th, the arrangement having remained untouched during the holidays. From this date until January 10th inclusive, the key was taken out before beginning experiments in the morning: there was no peculiar *first effect*; while, on the other hand, an *average effect* denoting a decrease of resistance came out very prominently. On January 12th and 13th the key was only taken out before magnetizing, and on these occasions the *first effect*, denoting increased resistance, was sufficiently marked.

Our method of procedure was varied in the above manner up to January 27th; and it was invariably found that, whenever the key was taken out before commencing experiments, there was no *first effect*; but when it was kept in until before magnetizing, this *first effect* was sufficiently marked. These experiments concur in proving that the *first effect* has some reference to the previous treatment of the wire; but they do not prove that it is at the same time connected with the putting on of the magnetism. To determine this point we made a set of experiments on January 22nd, 26th, and 27th. When the current had become constant the key was taken out, but the magnetism was not put on; and on these occasions there was no *first effect* of the current upon itself in the direction of increased resistance, but rather in the opposite direction. It thus appears that the *first effect* which increases the

resistance has not only reference to the previous treatment of the wire, but depends also upon the magnetism being put on.

This result is confirmed by experiments made previous to Dec. 12th, in which the key was not taken out at all. For instance, we have on Dec. 9th,

First off.	On first effect.	Second off.
0	+54	+45

We have hitherto only spoken of the *first effect* obtained after January 7th; we now come to the *average effect*. From January 7th to January 27th inclusive, the magnetism was always put on in the direction (1), and the *average effect* invariably denoted a decrease of resistance when the magnetism was on.

5. On January 28th the magnetism was reversed; the effect during this day was very irregular. On January 29th, 30th, 31st, and February 2nd the key was left in until before magnetization. The *first effect* was now extremely large; but it was suspected that, during these experiments, the contact of the key was not very good.

On January 29th the *average effect* denoted a decrease of resistance; but on January 30th, 31st, February 2nd, 4th, 6th, the *average effect* denoted an increase of resistance.

6. From February 6th until February 11th the wires were left broken; on February 11th there was a very slight *first effect* in the direction of increased resistance, and a slight *average effect* in the direction of decreased resistance. On February 12th a mercury interruptor was used instead of a metal key, both the wires being broken by it, and its use was continued until February 18th. The interruptor was left in over night and the current was only broken before magnetization, but no *first effect* was observed.

From February 19th to February 26th one wire only was broken by the fluid interruptor; nevertheless there was no *first effect*.

On February 12th, when the fluid interruptor was first employed, there was a very small *average effect* in the direction of increased resistance; but, in all the experiments afterwards, this *average effect* was in the direction of decreased resistance. The magnetism had been in the direction (2) from January 28th; but, during the experiment of February 25th, it was reversed and retained in this condition through the experiment of February 26th, without appearing to affect the results.

7. From these experiments we may perhaps conclude as follows:—

In the *first place*, there is a *first effect* in the direction of increased resistance which appears to have reference to three things—namely, the previous state of the wire, the solidity of the circuit, and its magnetization.

In the *second place*, we have an *average effect*, of which the normal state appears to denote a decreased resistance while the magnetism is on, without reference to the direction of the magnetism.

In the *third place*, when, in a solid circuit, the direction of the magnetism has been recently changed, there appears to be a temporary reversal of the average effect, which appears, at first, as an increase of resistance. Besides the evidence herein detailed, we have other evidence in favour of the third conclusion; for in some preliminary experiments, in which we frequently reversed the poles, we found an increase of resistance when the magnetism was on. We have given, in a Table appended to this paper, a synopsis of our various experiments.

8. We are led to conclude, from other experiments besides these, that the effect of the magnetism is not merely confined to the part of the copper wire wound round the poles, but is propagated all along the wire. On December 2nd, for instance, the current was passed through the wire, the galvanometer being joined as a secondary circuit. The main current was therefore measured.

The deflections were as follows:—

297	off	300	off
300	(1)	302	(1)
297	off	301	off
300	(1)		

This shows an average strengthening of the current, equal to about one two-hundredth part of the whole. Were this strengthening due to merely the change of resistance of that part of the wire wound round the poles, the effect, as measured by the much more delicate arrangement of Wheatstone's bridge, would be much larger than was actually observed.

9. Allusion was made in article 7 to some preliminary experiments, in which increased resistance was observed when the magnetism was put on (1) and (2) alternately. Similar experiments were made, giving the same result with a piece of coke and graphite, which were placed between the poles of the magnet.

10. We have also some evidence that a copper wire, one end of which is wound round the pole of the magnet, changes its position in the electromotive series. Two copper wires were dipped into dilute nitric acid and connected with the galvanometer. A weak current passed through the galvanometer owing to a slight difference in the copper wires, one of which was also connected with the copper wire wound round the magnet. When the magnet was on, the current, as a rule, changed in intensity; but the effect was small, and the difficulty of having two copper wires which, when joined together and dipped into nitric acid, give a current sufficiently weak and constant, prevented us from getting any decided results.

11. In conclusion we have to state that we regard these results which we have ventured to bring before the Royal Society as preliminary, the correctness of which will, we trust, be confirmed by the further experiments which it is our intention to make.

Date.	Nature of Experiment.	Value of first effect.	Number of observations.	Mean effect, excl. of first effect (—denote decrease of resistance).
1873.				
Dec. 17.	Metal key left in over night taken out before magnetism was put on (1)	off. on. off. 0+34+17	15	-14
" 18.	Metal key left in over night taken out before magnetism was put on (1)	0+54+24	30	- 9
" 19.	Metal key left in over night taken out before magnetism was put on (1)	0+33-18	15	+21
1874.				
Jan. 7.	The key was taken out before beginning the experiments in the morning	No first effect.	15	-17
" 8.	The key was taken out before beginning the experiments in the morning	Ditto	15	-19
" 9.	The key was taken out before beginning the experiments in the morning	Ditto	15	-13
" 10.	The key was taken out before beginning the experiments in the morning	Ditto	15	-20
" 12.	The key was not taken out until before magnetizing	0+47+18	15	-18
" 13.	The key was taken out (same as Jan. 7)	No first effect.	Irregular	action.
" 14.	The key was not taken out (same as Jan. 12) ...	0+3-6	15	-19
" 15.	Ditto	0+17+11	15	- 2
" 16.	The key was taken out (same as Jan. 7)	No first effect.	15	-67
" 17.	The key was not taken out (same as Jan. 12) ...	0+28+119	15	-22
" 20.	The connexions had been broken since Jan. 17 .	No first effect.	15	-44
" 21.	The key was taken out (same as Jan. 7)	0+7+1	15	- 9
" 22.	When the current was constant the key was taken out, but the magnet was not put on before the current had again become constant.....	No first effect.	15	-13
" 24.	The key had been left out since Jan. 22	Ditto	15	-11
" 26.	Same as Jan. 22	Ditto	15	-33
" 27.	Ditto	Ditto	15	-23
" 28.	The magnet was put on (2) from this day until Feb. 25
" 29.	The magnet was put on (2), key left in (same as Jan. 12)	0+102+47	15	-23
" 30.	The magnet was put on (2), key left in (same as Jan. 12)	0+219	15	+25
" 31.	The magnet was put on (2), key left in (same as Jan. 12)	0+137+161	15	+16
Feb. 2.	The magnet was put on (2), key left in (same as Jan. 12)	0+ 47+84	15	+11
" 3.
" 4.	The key was taken out and put in after several offs, same as Jan. 22.....	No first effect.	15	+31
" 5.
" 6.	The key was taken out (same as Jan. 22)	No first effect.	15	+ 3
" 11.	0+7+3	15	- 3
" 12.	A mercury interruptor was used instead of metal key	No first effect.	31	+ 3
" 13.	The mercury interruptor was kept in over night	Ditto	15	-11
" 14.	The mercury interruptor was kept in over night	Ditto	15	-36
" 16.	New mercurial contact breaker from Elliott Brothers used	Ditto	15	-18
" 17.	Ditto key left in (same as Jan. 12)	Ditto	15	- 5
" 18.	Ditto Ditto	Ditto	15	- 7
" 19.	One wire only was broken	0+11+16	15	- 2
" 20.	One wire only was broken, key left in (same as Jan. 12)	No first effect.	15	-13
" 22.	One wire only was broken, key left in (same as Jan. 12)	Ditto	17	-12
" 24.	One wire only was broken, key left in (same as Jan. 12)	Ditto	17	- 8
" 23.	One wire only was broken, magnet on (1)	Ditto	15	- 4

REMARKS.

- Dec. 18, 1873. For a second series of 15 on this day no first effect was found.
- Jan. 15, 1874. There was a sudden change of the current during the experiments, to which the unusually small effect is most likely due.
- Jan. 16. There was a sudden change of the current during the experiments, to which the unusually large effect is most likely due.
- Jan. 17. There was an irregularity at the beginning of the experiment.
- Jan. 20. Action somewhat irregular.
- Jan. 22. There seemed to be a first effect of the current on itself in the opposite direction, 0—14—9.
- Jan. 26. There seemed to be again a first effect in opposite direction, 0—47—57.
- | | | | |
|----------|-------|-------|----------|
| Jan. 27. | Ditto | Ditto | 0—17—17. |
|----------|-------|-------|----------|
- Jan. 28. The action was very irregular.
- Jan. 29. It is suspected that during the experiments from Jan. 29 to Feb. 12 the contact at the key was not very good.
- Feb. 3. The action was very irregular.
- Feb. 4. There seemed to be two first effects of the current upon itself in the direction of increased resistance.
- Feb. 5. The action was very irregular.
- Feb. 6. There seemed to be a first effect of decreased resistance of current upon itself.
- Feb. 11. The wires had been broken since Feb. 6th.
- Feb. 12. One of the wires had got between the pole and the core of the magnet.
- Feb. 24. After the first on (2) the magnet was always put on (1).

GEOLOGICAL SOCIETY.

[Continued from p. 313.]

January 21, 1874.—Prof. P. Martin Duncan, F.R.S., Vice-President, in the Chair.

The following communications were read:—

1. Extract of a Despatch from Mr. Williams, H.M.'s Consul at Samoa, dated Sydney, Oct. 28, 1873. Communicated by H.M.'s Secretary of State for Foreign Affairs.

"The afternoon before I left [Samoa], samples of gold in quartz were put into my hands, found by three Englishmen in a valley about three miles from the port of Apia; but not having visited the spot I cannot vouch for the discovery, but I have every reason to believe that the gold is there. I have had the samples assayed here, and the yield is at the rate of 3000 ozs. to the ton."

2. "The Secondary Rocks of Scotland.—Second Paper. On the Ancient Volcanoes of the Highlands and their Relations to the Mesozoic Strata." By J. W. Judd, Esq., F.G.S.

Introduction.—The vestiges of the Secondary strata on the west coast of Scotland have been preserved, like the interesting relics of Pompeii, by being buried under the products of volcanic eruptions. The deposition of the Mesozoic strata in this district was both preceded and followed by exhibitions of volcanic phenomena on the grandest

scale ; and it is only by a careful study of the records of these two great periods of igneous activity that we can hope to understand the remarkable relations of the fragments of the intermediate sedimentary formations, or to account for the peculiarities which they present.

That the rocks forming the great plateaux of the Hebrides and the north of Ireland are really the vestiges of innumerable lava-streams, is a fact which has long been recognized by geologists. That these lavas were of *subaerial* and not *subaqueous* origin, is proved by the absence of all contemporaneous interbedded sedimentary rocks, by the evidently terrestrial origin of the surfaces on which they lie, and by the intercalation among them of old soils, forests, mud-streams, river-gravels, lake-deposits, and masses of unstratified tuffs and ashes. From the analogy of existing volcanic districts, we can scarcely doubt that these great accumulations of igneous products, which must originally have covered many thousands of square miles, and which still often exhibit a thickness of 2000 feet, were ejected from great volcanic mountains ; and a careful study of the district fully confirms this conclusion, enabling us, indeed, to determine the sites of these old volcanoes, to estimate their dimensions, to investigate their internal structure, and to trace the history of their formation.

The Tertiary Volcanoes.—The petrology of the Western Isles has been made the subject of careful study by Professor Zirkel, of Leipzig, to whose investigations we are very deeply indebted. The Tertiary igneous rocks may be classified, according to their ultimate chemical composition, into two series, known as the acid and basic igneous rocks. In each of these series the proportions of the several ingredients in its various members are almost identical ; but in structure the rocks of either series vary from the coarsest crystalline aggregates to the most perfect glass. The acid series consists of granite, felsite, felstone, and pitchstone ; the basic of gabbro, dolerite, basalt, and tachylite ; the members of either series exhibit innumerable varieties, and pass into one another by the most insensible gradations. The igneous rocks of both classes form :—*lava-streams*, often of great thickness and extent, and exhibiting many interesting peculiarities of the amygdaloidal and columnar structures ; *eruptive masses*, varying in size from great mountain-groups to the smallest dykes and veins ; *volcanic agglomerates*, composed of the scorïæ and ashes ejected from volcanic vents ; and *volcanic breccias*, made up in great part of the fragments of the various Palæozoic and Secondary rocks through which the volcanoes have burst. Among the volcanic agglomerates are found beautiful examples of the more stable of the species of minerals characteristic of the neighbourhood of volcanic vents.

The relations of these several igneous products to one another are beautifully exemplified in the Island of Mull. We here find proof that the volcanic activity of the Tertiary period commenced with the eruption of felspathic lavas and associated fragmentary materials.

These were accompanied by the intrusion among the surrounding strata, which they greatly metamorphosed, of great masses of fluid rock of acid composition, and the extrusion among the other igneous products of similar liquefied materials, which consolidated into felsite and granite. The great volcanic mountains thus formed appear to have remained in a state of comparative quiescence for a vast period, during which they were subjected to great denudation, and then through their midst were forced great masses of fluid basaltic rocks, which continued to flow at intervals during enormous periods, and gave rise to streams of basalt which accumulated to the thickness of many hundreds and even thousands of feet. The great intrusive bodies of this same rock consolidated into mountain masses of gabbro and dolerite. While the earlier felspathic lavas appear to have rarely flowed to a distance of more than ten miles from the volcanic vent, those of basaltic character often extended to distances of fifty or sixty miles, or even more. The same difference of behaviour of the two classes of lava has often been remarked in the case of existing volcanoes.

Besides the volcano of Mull we have evidence of the existence of four other great vents in the northern part of the Hebrides—namely, in the peninsula of Ardnamurchan, and in the islands of Rum, Skye, and St. Kilda respectively. In each of these a period characterized by the eruption of felspathic lavas was followed, after a considerable interval, by one during which nearly all the materials thrown out were of basaltic composition. The volcano of Mull is in a far more perfect state of preservation than the others, owing to the great amount of central subsidence which has taken place in its mass. This central subsidence appears to be strictly analogous to that which has been shown by Mr. Darwin, Mr. Heaphy, and Krug von Nidda to have occurred in the case of recent volcanoes in the Cape-Verd Islands, New Zealand, and Iceland. From an examination of the areas covered by the great Tertiary volcanoes of the Hebrides, and of the interesting data afforded by the present positions of their lava-streams &c., we are able to estimate that while the volcanoes of Mull and Skye were certainly of far greater bulk than Etna, those of Ardnamurchan, Rum, and St. Kilda could have been of scarcely inferior dimensions.

There is proof that after the extinction of the five great volcanoes of the Northern Hebrides and the very extensive denudation of the great plateaux composed of their lavas, there burst out a number of sporadic eruptions which resulted in the formation of comparatively small volcanic cones, analogous to the “pays” of Central France, which have been so admirably described by Mr. Scrope. These “pays” of the Hebrides are very numerous, and are exhibited to us in various stages of preservation.

The formation of the various volcanic piles of the Western Isles was accompanied by the intrusion of innumerable igneous masses of all sizes among the surrounding older strata. The liquefied rocks of acid composition accumulated in great lenticular masses in the midst of the stratified rocks, consolidating into felsite and granite :

while the heavier and more fusible basaltic materials spread between the strata in vast sheets of enormous extent, which, when cooled, formed dolerite and basalt. Besides the larger intrusive masses, the whole district around each of the volcanic vents is traversed by a wonderful plexus of dykes and veins, composed of both acid and basic rocks, some of the dykes of basic composition extending to extraordinary distances, as pointed out by Prof. Geikie. The great igneous masses, besides *disturbing* the older strata through which they have been forced, have effected a remarkable *metamorphism* in them, the amount of this metamorphism and the distance to which it extends being in each case proportioned to the bulk of the intrusive mass.

From a consideration of the whole of the evidence it appears highly probable that the first period of igneous activity (namely, that of the eruption of felspathic lavas from the great volcanoes) was contemporaneous with the Eocene sedimentary formations; the second period, that of the great and prolonged outbursts of basaltic lavas from the same vents, was certainly that of the Miocene; while the third period, or that of the formation of the "puys," may, with a great show of probability, be correlated with the Pliocene.

The igneous activity during the Tertiary period in the Northern Hebrides appears to have extended in all its magnitude, and to have exhibited similar stages in its development, far to the southwards, as is illustrated by the rocks of Arran, Antrim, and the Mourne Mountains. But even this tract, extending 400 miles from north to south, which was characterized by grand volcanic phenomena during the whole of the Tertiary periods, can only be regarded as a portion of the great belt of volcanoes which at that epoch extended through Greenland, Iceland, the Faroe Islands, the Hebrides, Ireland, Central France, the Iberian peninsula, the Azores, Madeira, Canaries, Cape-Verd Islands, Ascension, St. Helena, and Tristan d'Acunha, and which constituted, as shown by the recent soundings of H.M.S. 'Challenger,' a mountain-range comparable in its extent, elevation, and volcanic character with the Andes of South America. The admirable manner in which the relations between the Volcanic and Plutonic rocks are exhibited in the old volcanoes of the Hebrides renders them of special interest to the geologist; and the further illustrations of the same phenomena which are afforded to us by the relics of a still older series of volcanoes, are made more clear and striking by the aid of their analogies.

The Newer Palæozoic Volcanoes.—In the district of Lorne we find a great series of old felspathic lavas which, in their lower part, alternate with conglomerates and sandstones, and which, in their higher portions at least, appear to be of subaerial origin. It is evident that we have here the relics of what was once a widely spreading plateau made up of lava-streams, like that of Tertiary age already described. These rocks were evidently formed long subsequently to the Lower Silurian strata, but before any of the Secondary sediments were deposited.

The central and southern districts of Scotland exhibit enormous

masses of igneous rocks, in part at least of subaqueous origin. These exhibit a very close similarity in petrological character to the lavas of Lorn, and are shown, by the interbedded and contemporaneous fossiliferous sediments associated with them, to range in age from the Lower Old Red Sandstone to the Lower Carboniferous.

Along the whole line of the Grampian Mountains we find a number of granitic masses connected with a wonderfully complicated series of veins and dykes of rocks of similar composition. These igneous intrusions, which disturb and metamorphose the surrounding strata, are evidently, as shown by Murchison and Geikie, of far later date than the Lower Silurian, but are earlier than the Secondary strata.

Concluding, as we cannot avoid doing, that these igneous intrusions and the subaerial and subaqueous lavas of similar composition were all formed during the Newer Palæozoic periods, we are led to the presumption of their probable former connexion with one another. By the phenomena presented at a number of interesting points, such as Ben Nevis and Glencoe, where the granitic rocks and the lavas are so associated with one another and with masses of volcanic agglomerate as to demonstrate the identity of their origin with that of the similar masses of Tertiary age in the Hebrides, this presumption is converted into certainty. The Newer Palæozoic period of volcanic eruption terminated, like that of the Tertiary epoch, by a grand development of "puys" during the Carboniferous and Permian periods. Of these, the celebrated Arthur's Seat, near Edinburgh, and many similar cases in Fife and the Lothians may be cited as examples.

Conclusion.—It appears that during the Newer Palæozoic and the Tertiary periods, the north-western parts of the British archipelago were the scene of displays of volcanic activity upon the grandest scale. During either of these, the eruption of felspathic lavas &c. preceded, as a whole, that of the basaltic; and, in both, the volcanic action was brought to a close by the formation of "puys." The range of Newer-Palæozoic volcanoes arose along a line striking N.E. and S.W., that of the Tertiary volcanoes along one striking from N. to S.; and each appears to have been connected with a great system of subterranean disturbance. It is an interesting circumstance that the epochs of maximum volcanic activity, the Old Red Sandstone and the Miocene, appear to have been coincident with those which, as shown by Prof. Ramsay, were characterized by the greatest extent of continental land in the area.

The Secondary strata were deposited in the interval between the two epochs of volcanic activity; and the features which they present have been largely influenced by this circumstance. Apart from this consideration, however, the volcanic rocks of the Highlands are of the highest interest to the geologist, both from their enabling him to decipher to so great an extent the "geological records" of the district, and from the light which they throw upon some of the obscurest problems of Physical Geology.

3. "Remarks on Fossils from Oberburg, Styria." By A. W. Waters, Esq., F.G.S.

The author noticed the limited occurrence of Eocene deposits in
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Styria, and referred briefly to the researches of Prof. Reuss and Prof. Stur upon them. He then indicated certain species of fossils which he had detected in these beds, adding about nine species to Stur's list.

LXXIII. *Intelligence and Miscellaneous Articles.*

ON THE COSMIC DUST WHICH FALLS ON THE SURFACE OF THE EARTH WITH THE ATMOSPHERIC PRECIPITATION. BY A. E. NORDENSKIÖLD*.

ON the occasion of an extraordinary fall of snow, which took place at Stockholm in the first days of December 1871, M. Nordenskiöld was curious to ascertain if the snow, though apparently pure, did not contain solid particles involved in its fall. When snow had already been falling for several days, and must therefore have removed from the atmosphere the greater part of the impurities which it might contain, he collected upon a sheet a cubic metre of fresh snow, which left on fusion a small solid residue. *This consisted of a black powder resembling coal. Heated, it gave liquid products of distillation; calcined, it was reduced to a brown-red ash. Moreover it contained a number of metallic particles attracted by the magnet and giving all the reactions of iron.*

The experiment having been made in the vicinity of a large city, was not sufficiently conclusive; and it was important to repeat it under other conditions, at a distance from every human habitation and industry. This was done by Professor Nordenskiöld in the Swedish polar expedition in 1872, which was detained by the ice as early as the commencement of August in about the 80th degree of north latitude, before reaching Parry's Island to the north-west of Spitzbergen, where it was to winter.

The examination of the snow which covered the icebergs, having evidently come from still higher latitudes, showed that it was strewn with a multitude of minute black particles, spread over the surface or situated at the bottom of little pits, of which the upper layer of snow presented a great quantity, or, again, lodged in the lower strata. This powder, which became grey on drying, included a large proportion of metallic particles attracted by the magnet, and became coated with copper by immersion in sulphate of copper.

An observation made a little later upon other icebergs proved the presence of a powder absolutely identical, in a layer of granular crystalline snow situated at some centimetres depth beneath a layer of light fresh snow and a second layer of hardened snow.

In the course of the expedition the author was able to collect several milligrammes of this substance, which on chemical analysis was found to contain metallic iron, phosphorus, cobalt, and probably nickel, with a residue insoluble in chlorhydric acid, and containing, among other things, fragments of Diatomaceæ.

The powder thus collected on the polar sea to the north of Spitz-

* Poggendorff's *Annalen*. 1871, vol. ch. p. 154.

bergen exhibits the greatest analogy with the dust previously observed by M. Nordenskiöld on the snows of Greenland, and described by him under the name of *kryokonite*. These two precipitates have probably a common source, at least as regards their metallic and magnetic portion, composed of iron, cobalt, and nickel, the cosmic or meteoric origin of which does not appear contestable.

M. Nordenskiöld has also found analogous ferruginous particles in hailstones collected at Stockholm. It seems, therefore, well established by these various observations, that in the polar regions there often falls with the atmospheric precipitation a cosmic dust containing the metals iron, cobalt, and nickel, phosphoric acid, and a carbonaceous organic powder. M. Nordenskiöld remarks on the interest this discovery may have for the theory of shooting-star showers, aurore boreales, sun-haze, &c., and on the part which may be played in the economy of our globe by this importation, slight, it is true, but perpetual, of new matter to its surface.

Before quitting this interesting subject, we will remind our readers of the results recently obtained by M. Tissandier in his study of the atmospheric dust gathered at Paris*, which was always found to contain a notable quantity of iron.—*Bibliothèque Universelle, Archives des Sciences, Phys. et Nat.* Nov. 15, 1874, pp. 282–284.

ON THE PASSAGE OF GASES THROUGH LIQUID FILMS.

BY DR. F. EXNER.

That gases can penetrate a soap-film has been already shown by Draper and Marianini, but without making quantitative determinations or going into the causes of the phenomenon. In the present memoir are contained quantitative determinations with air, illuminating gas, N, O, H, CO², H²S, and NH³. From these is deduced the law that the velocities of diffusion of gases are proportional to the expression $\frac{C}{\sqrt{\delta}}$, in which C signifies the coefficient of absorption of the gas for the liquid of which the film consists, and δ the density of the gas. For the diffusion-velocities of the gases examined, putting that of air = 1, the following numbers were found

variation :—
 N = 0.86 Illuminating gas = 2.27 CO² = 47.1 NH³ = 46000
 O = 1.95 H = 3.77 H²S = 165

Within these extraordinarily wide limits, between 0.86 and 46000, the observations are in perfect accord with the formula $\frac{C}{\sqrt{\delta}}$. Experiments were also made for the purpose of ascertaining the absolute velocity of diffusion. These gave for the diffusion of H in air through a soap-film, that in 1 minute 1.88 cubic centim. H and 0.50 cubic centim. air simultaneously pass through 1 square centim. of the film.—*Kaiserliche Akademie der Wissenschaften in Wien, Sitzung der math.-nat. Classe*, November 5, 1874.

* *Comptes Rendus de l'Acad. des Sciences*, March 23, 1874.

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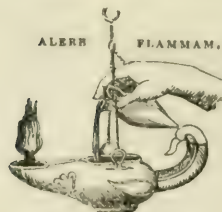
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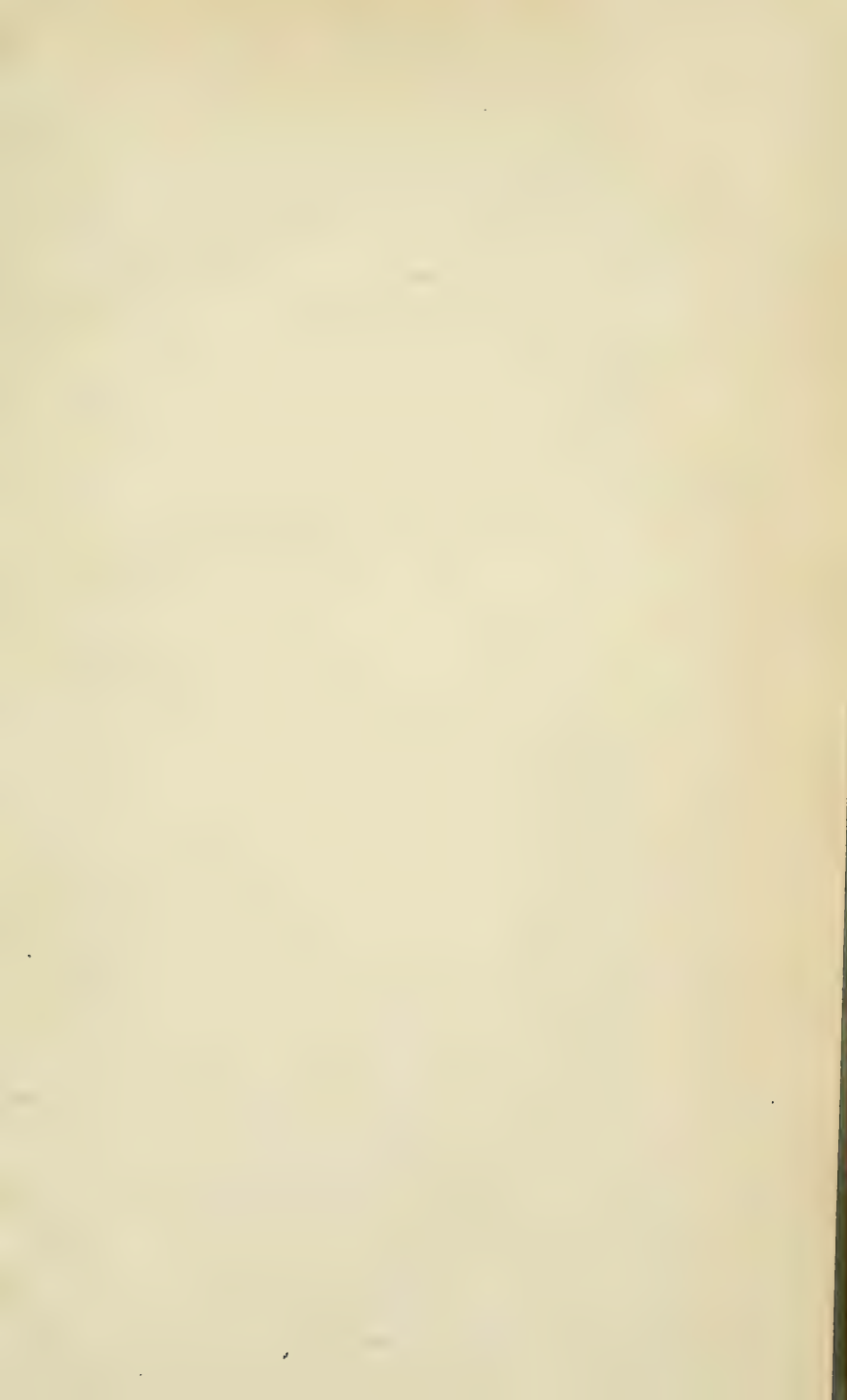
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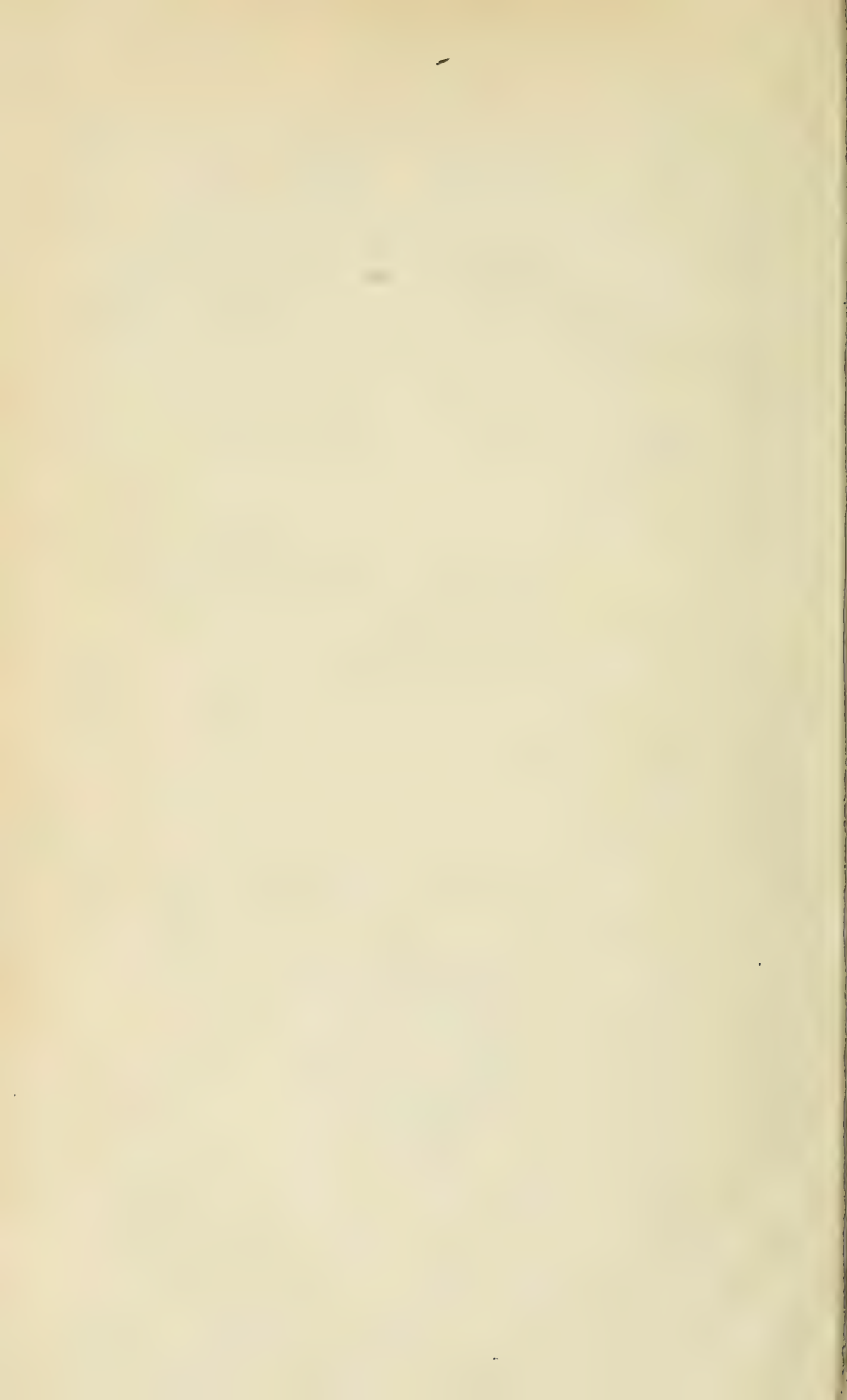
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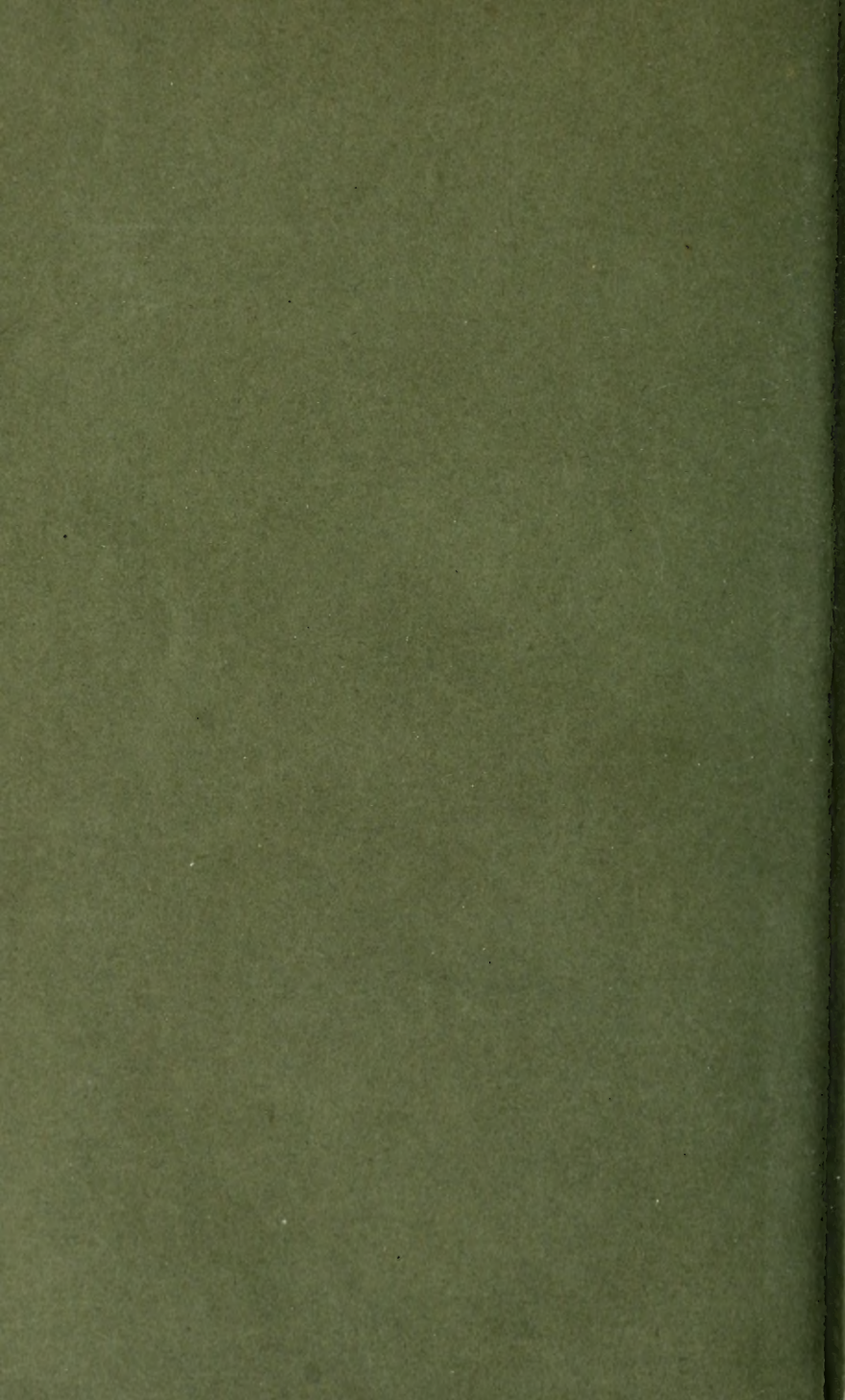
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